# Violence Against Women at Work*

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**Abstract**

Between-colleague conflicts are common. We link every police report in Finland to administrative data to identify assaults between colleagues, and economic outcomes for victims, perpetrators, and firms. We document large, persistent labor market impacts of between-colleague violence on victims and perpetrators. Male perpetrators experience substantially weaker consequences after attacking women compared to men. Perpetrators' economic power in male-female violence partly explains this asymmetry. Male-female violence causes a decline in women at the firm. There is no change in within-network hiring, ruling out supply-side explanations via "whisper networks". Only male-managed firms lose women. Female managers do one important thing differently: fire perpetrators.

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# 1 Introduction

Workplace violence is sufficiently common to warrant its own catchphrase in popular culture, "going postal".[1] While many men are victims of workplace violence, there is often a gendered aspect to these crimes. The United States Bureau of Labor Statistics identified more than 20,000 cases of workplace violence in 2019; in 68% of cases, victims were female.[2] The #MeToo movement made women's experiences of violence at work salient, with many accounts characterized by high-profile men in positions of power attacking subordinates with few repercussions.

There is little empirical research on the impact of workplace violence on perpetrators, victims, and the wider workforce. The nascent economics literature on workplace sexual harassment has largely focused on less serious crimes. Folke and Rickne (2022) find that workers in a firm's gender minority are more likely to experience sexual harassment, and that workers are willing to pay approximately 10% of their wages to avoid harassment, suggesting potentially large costs to victimization. While a recurring theme in #MeToo accounts is that perpetrators are rarely held accountable, there is no literature estimating the consequences that perpetrators face for assaulting a colleague at work. Is it only the rich and powerful who go unpunished, or do unremarkable managers in nondescript offices also enjoy less severe consequences if they assault a subordinate?

In this paper, we harness unique Finnish administrative data to analyze the impact of workplace violence on victims, perpetrators and the broader firm. We link information on every police report in Finland between 2006-2019 to administrative records on employment, income, and demographic characteristics.[3] This allows us to identify violent incidents where both the perpetrator and victim worked in the same plant (hereafter the "firm") at the time of an incident. For each crime, we observe the economic outcomes for victims, perpetrators and the firm.[4] While police reports will undoubtedly miss the true scope of violent incidents amongst colleagues as

[1]This expression originated in a 1993 newspaper article after a series of shootings by employees at postal offices in the 1980s. The phrase is now used to describe becoming angry or violent in a workplace environment.

[2]See https://www.cdc.gov/niosh/topics/violence/fastfacts.html.

[3]A police report initiates an investigation, and occurs before a suspect is formally charged with a crime or a court case.

[4]This contrasts to small literatures on workplace violence in applied psychology and sociology which primarily use survey evidence to examine victims. See discussion in Section 2 for more details.

it is likely that most cases of (minor) violence go unreported, they provide an important step in understanding this phenomenon.[5]

We identify over 5,000 cases of violence between colleagues. 57% of incidents between employees from the same firm are assaults or petty assaults, and the remaining 43% are a mix of negligent bodily injury, menace, and other crimes. The vast majority (83%) of perpetrators are men, while victims are evenly split between male and female. Reported workplace violence with a male perpetrator and female victim (hereafter male-female) are much more likely to be assaults or menace, and thus more serious in nature, compared with workplace violence with male perpetrators and male victims (hereafter male-male). Compared to all firms in Finland, firms where violent incidents occur are larger and pay higher average wages. Firms that experience male-female violence have a similar share of female employees before the incident and are approximately as likely to be female managed compared to all firms in Finland.

We first analyze the impact of colleague violence on victims' and perpetrators' outcomes. To address concerns that poor labor market outcomes cause an individual to commit violence against a colleague, or make a colleague more exposed to abuse, we take a matched control event study approach similar to Schmieder *et al.* (2022). We identify victims' and perpetrators' nearest neighbour match on a rich set of individual and firm outcomes over the five years before an incident. We show that income and employment evolve identically between those involved in a violent incident and their counterfactual matched control observation preceding the incident.

Following a violent incident, victims experience an immediate drop in employment that persists at least five years following the incident. Perpetrators experience similarly large and persistent negative impacts.[6] However, there is a dramatic asymmetry in the impact of colleague violence on perpetrators and victims for male-female crimes versus male-male crimes.[7] For male-male crimes results are as one might expect: perpetrators experience significantly greater neg-

[5]We discuss the implications of under-reporting for the interpretation of our estimates in Sections 2 and 7.

[6]Our results are robust to dropping some of the pre-periods when matching and to using future victims/perpetrators as the counterfactual without matching. Moreover, a placebo exercise estimating impacts 5 years prior to the event when no violence takes place shows no significant impacts.

[7]We do not separately analyze female perpetrated workplace violence since a) women rarely attack colleagues resulting in small sample sizes and b) in the majority of cases where a women is recorded as the perpetrator, she is also recorded as a victim, thus these are not clear-cut cases of female perpetrated violence. See Section 2.2 for details.

ative repercussions than their victims. Employment rates fall by 10.6 percentage points for perpetrators and 4.2 percentage points for victims in the five years after a violent incident between men. Results are, however, very different for male-female crimes: while perpetrator employment falls by only 5.2 percentage points on average in the five years following an incident, victim employment falls by 8.4 percentage points.

We show that economic inequality between perpetrators and victims plays a key role in accentuating or mediating the impact of colleague violence on outcomes. We interact the treatment of an attack by a colleague with indicators for whether the perpetrator is a manager and the difference in the income rank of perpetrators and victims within the firm. We find that for male-female (male-male) crime, victims' employment rates fall by 5.6 (7.4) percentage points *more* when their perpetrator is a manager. However, perpetrators who are managers are 5.9 (13.3) percentage points *less* likely to be unemployed in the five years following an incident. Results are similar when using income gaps as the measure of relative power. Thus, perpetrator power plays an important role in determining the impact of violence, and partially accounts for the comparatively smaller labor market impacts experienced by perpetrators of male-female violence where such power imbalances are more common.

Next, we investigate the broader implications of colleague violence for the firm. We find that male-female violence has systematic effects on the gender composition of the workplaces in which it occurs. Following an incident, the gender composition of firms becomes significantly more male.[8] This fall in the female share of employees is entirely explained by male-female violence; there is no significant impact of male-male violence on the gender composition of employees. The reduction in the share of employees who are women in firms where male-female violence takes place is explained both by higher separation rates of existing female employees and a significant reduction in the share of women amongst new hires.

While the rise in female exits following violence is relatively straightforward to interpret, falls in the proportion of women hired by the firm could be driven by supply side factors, i.e. women being less likely to apply for jobs at the firm, or by demand side factors, i.e. women being less

[8]This is true even when excluding victims and perpetrators from the analysis.

likely to be hired from a given set of applicants. We do not directly observe applicants to positions. Thus, to investigate whether a supply side explanation is operative, we analyze changes in hiring rates from existing employee networks. A large literature demonstrates that new hires are often recruited from within existing employee networks (Bayer *et al.*, 2008; Burks *et al.*, 2015; Beaman and Magruder, 2012; Brown *et al.*, 2016; Dustmann *et al.*, 2016), with over 30% of employees finding jobs through personal networks (Barwick *et al.*, 2019). These informal networks are the most likely way individuals learn about colleague violence, given that perpetrator names in police reports are not public. This type of supply side mechanism would be consistent with the idea of "Whisper Networks", a term popularized during #MeToo to reflect the informal dissemination of information amongst women regarding bad men and bad firms to avoid.

We construct the network of siblings and past colleagues from the previous ten years for every employee at the violent and matched control firms (Hensvik and Skans, 2016). We find zero impact on within network hiring for both types of networks. These results suggest that informal approaches of information sharing, which have been touted as a means by which women might avoid abuse and harassment at work, may not be very effective. Given this zero impact where we would expect the largest supply side response, we conclude that the drop in the share of female new hires is more likely explained by demand side factors, i.e. firms choose to hire fewer women following male-female violence.

In the final part of the paper, we focus on the role of managers in mediating the impacts on the wider workforce. Previous research demonstrates that managers help determine the success of a firm (Bertrand and Schoar, 2003; Bloom *et al.*, 2007, 2013, 2019; Bender *et al.*, 2018; Bandiera *et al.*, 2020; Gosnell *et al.*, 2020). Moreover, there is important heterogeneity in how male and female decision makers interpret and respond to negative (or positive) shocks, and how the gender of the individual responsible for the shock might change the response of the manager (Chakraborty *et al.*, 2021; Sarsons, 2017). In particular, Egan *et al.* (2022) show that women found guilty of misconduct are more likely to be fired, and this is entirely explained by male-managed firms.

Motivated by these facts, we consider heterogeneity in the impacts of workplace violence by the proportion of women in decision-making positions. Following Bender *et al.* (2018), we calculate the proportion of women in the top 20% of earners in the firm. We find that the reduction of women in the workforce is isolated to male-managed firms, i.e. those that have a below-median share of women in high earning positions relative to the rest of their industry. We find that female managers are more likely to fire perpetrators of workplace violence and this appears to be a key mediating factor for women in the broader firm, although the gender composition of management does not significantly affect the direct victims' labor market outcomes. We show that it is perpetrators losing their jobs at female managed firms, rather than female management in general, that mitigates the consequences of workplace violence on the wider workforce.

Our findings contribute to three broad literatures. Most closely related to our paper is a small but growing literature showing that women disproportionately experience costly interactions with peers at work (Hersch, 2011; Antecol and Cobb-Clark, 2006; Basu, 2003; Batut *et al.*, 2021).[9] In particular, we complement Folke and Rickne (2022) who show that women are more likely to experience sexual harassment on the job, that harassment is more likely when women are in the minority, and that harassment is associated with higher turnover of those affected. They also show through randomized job offer vignettes in a survey setting that workers are willing to give up approximately 10% of their wages to avoid workplace sexual harassment. This revealed preference to avoid harassment at work is consistent with our finding that there are very large costs to victimization, although their paper focuses on less severe forms of workplace harassment.

We complement this paper by estimating the impact of relatively serious, realized events of workplace violence for both male and female victims, as well as for the broader firm. Because of our unique data, we are able to link perpetrators and victims together, and examine how their relative economic standing mediates these impacts. We show that the large impacts for victims of male-female workplace violence, the relative lack of consequences for male perpetrators, and the importance of power dynamics hold true for violence against women at work in general, and not just for the high-profile cases of workplace violence reported in the media. We additionally show that these events have impacts that extend well beyond the perpetrator and the victim to the broader firm, and that management practices can play a role in mitigating these broader costs.

[9] Related is a literature documenting impacts of crime on victims in other contexts (Bindler and Ketel, 2022; Johnston *et al.*, 2018; Currie *et al.*, 2018; Koppensteiner and Menezes, 2021).

Second, our paper provides novel insights into the repercussions of extreme peer interactions at firms. Despite survey evidence suggesting that workplace violence is unfortunately common, this is the first empirical study of the phenomenon in economics that we are aware of. In so doing, our findings complement a large body of evidence that shows that peers can have large negative (or positive) spillovers on those they interact with not only in the workplace (Brune *et al.*, 2020; Papay *et al.*, 2020; Nix, 2020; Cornelissen *et al.*, 2017; Waldinger, 2012; Mas and Moretti, 2009; Thornton and Thompson, 2001), but also in schools (Carrell *et al.*, 2018; Black *et al.*, 2013; Hoxby, 2000), neighborhoods (Billings and Schnepel, 2020; Godlonton and Thornton, 2012; Bobonis and Finan, 2009), and even prison (Bayer *et al.*, 2009). Consistent with these papers, we not only find negative impacts on the most closely connected peer (the victim of the violent incident), but also more broadly on other workers in the firm as the composition of peers changes following a violent incident between workplace peers.

Finally, our paper makes an important contribution to the literature on firms and firm management on worker outcomes (Bertrand and Schoar, 2003; Bloom *et al.*, 2007; Bandiera *et al.*, 2007; Ichniowski *et al.*, 1995; Alan *et al.*, 2021). We show that extreme and toxic events in workplaces result in large costs for direct victims, especially female victims, cause other women in the firm to leave, and change the hiring patterns of firms. The gendered aspects of the firm managerial response to these events is most closely related to Sarsons (2017), Chakraborty *et al.* (2021), and Egan *et al.* (2022) described previously along with Benson *et al.* (2021) who show that male managers are less likely to promote female subordinates who are equally productive by scoring them lower in terms of potential and Cullen and Perez-Truglia (2019) who find that male managers are more likely to promote male subordinates.[10]

The paper is organized as follows. Section 2 describes the data, how we measure workplace violence, and provides descriptive statistics. Section 3 describes our empirical strategy. Section 4 presents impacts on victims and perpetrators. Section 5 presents impacts on firms. Section 6 explores the role of managers. Section 7 discusses implications of our results. Section 8 concludes.

[10]Also related is Stoddard *et al.* (2020) who shows that women who find themselves in the minority within a group in a professional setting are at a disadvantage and viewed less favorably by their peers.

## 2 Data and Descriptive Statistics

### 2.1 Data and Defining Workplace Violence

We use novel sources of Finnish administrative data to study the impact of between colleague violence on worker and firm outcomes. We observe the universe of police reports filed between 2006-2019, which we merge with Finnish Linked Employer-Employee Data (FLEED), i.e. population register data containing annual income, annual employment, and demographic characteristics. A police report is the first step in any investigation, and occurs before a perpetrator is formally charged with a crime, and before any court case takes place. 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. Only after this step can a court case take place. While court cases are public record, police reports are not.

Importantly, the police data contains individual identifiers for both the perpetrator and victim, and the employer-employee register data contains both individual identifiers and a unique identifier for the plant and firm at which a worker is employed. This allows us to identify police reports between colleagues employed at the same establishment.[11] Hereafter, we will refer to the "establishment/plant" as the "firm", as we will not focus on the broader firm identifiers (for example, we will focus on each individual McDonald's plant as the "firm" of interest as opposed to McDonald's as a whole).

We classify an incident recorded in a police report as workplace violence if both the victim and perpetrator worked at the same plant in the same year or in the year before an incident. We also include colleagues in the year before an incident because plant identifiers are only observed at the end of the calendar year. Suppose a violent incident among colleagues occurred in March 2013 leading to either the victim or perpetrator separating from the plant in April 2013. In this example, we would see the victim and perpetrator work in the same plant in December 2012 but not in December 2013.

After merging the police data with the rich administrative data on individual labor market

[11] Person, workplace, and firm identifiers are unique ensuring matches are perfect.

outcomes, we construct the labor market trajectories of victims and perpetrators before and after an incident. For victims and perpetrators, we focus primarily on employment as measured in December of each year. In the Appendix we also report impacts on income, which consists of all labor income reported to the tax authorities and measured in December of each year.[12] Moreover, we use the plant identifiers from the full population register data to construct relevant firm outcomes such as headcount, turnover, and the gender composition of the workforce. The plant identifiers also allow us to construct labor market outcomes of other workers employed at the same firm which we use in later analysis.

It is worth noting that the limited previous literature on workplace violence in other disciplines, such as sociology and psychology, almost solely relies on survey data and small selected samples. For example, an applied psychology literature suggests that violence and harassment can be a key workplace environment factor with implications for individual and organizational performance (Cortina *et al.*, 2001; Lim *et al.*, 2008; Estes and Wang, 2008; Geck *et al.*, 2017). Thus, the use of administrative data to study this question is new. This type of data not only allows us to examine direct impacts on victims, but makes it possible to examine outcomes that have never been studied in this context before, such as impacts on perpetrators (prior papers have focused almost exclusively on victims), the role of the within firm economic relationship between victim and perpetrator, impacts on the broader firm including workplace colleagues and new recruits, and the role of management.

**Measurement Discussion** Our measure of workplace violence captures the population of violent incidents that occur between two colleagues and are reported to the police. These incidents do not necessarily occur in the workplace itself: the police data does not include the precise location of the crime. This means that the incidents we study could be happening both inside and outside the official premises of the firm. However, we view this as an advantage of our data relative to an alternative scenario where one only observes violence occurring during working hours

[12] We focus on labor income, as we are interested in labor market impacts. We do not include all taxable income. However, it is possible to additionally examine alternative income streams reported to tax authorities, such as certain benefits and capital income.

within the four walls of the firm.[13] People can be assaulted by colleagues at off-premises holiday parties, when traveling for work, etc. Using our definition of workplace violence, all such incidents will be included. This means, however, that our measure will also capture domestic violence where partners work at the same firm. We do not view this as a major concern as we find that fewer than 2% of domestic violence incidents occur between colleagues. Nonetheless, in Section 4.3 we show that our results are robust to excluding domestic violence cases from the sample.

The primary limitation of our measure is that we are only able to observe incidents that are reported to the police. However, reporting is far from universal. 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; European Institute for Crime Prevention & Control, 2009). Our measure therefore understates the true prevalence of workplace violence and likely captures the most serious incidents. However, survey evidence suggests that reporting rates for non-partner assault are similar for male and female victims (EU Agency for Fundamental Rights, 2015; European Institute for Crime Prevention & Control, 2009). This evidence suggests that the coverage of our measure should not differ significantly by gender. We discuss the interpretation of our results in light of the incomplete reporting in Section 7 at the end of the paper.

### 2.2 Descriptive Statistics

Table 1 provides an overview of the types of crimes between colleagues in the police report data. In Table 2, we report summary statistics for the characteristics of victims and perpetrators of between-colleague violence in the year before an incident. Table 3 gives the characteristics of firms in which violence takes place and compares their characteristics to the rest of firms in Finland. Note that these tables are constructed before imposing the estimation sample restrictions from Section 3. Between 2006-2019, there were over 5,200 police reports of violence between colleagues in Finland. Table 1 shows that the majority of these incidents were one of four crime

[13] For example, Harvey Weinstein was known to attack assistants in hotel rooms, and generally not while at his formal offices.

types: assault (36.8%); petty assault (20.3%); menace (13.8%); and negligent bodily injury (13.5%).

**Table 1:** Crime Types

| Crime types | All | | | Male-Female | | Male-Male | |
|---|---|---|---|---|---|---|---|
| | Number (1) | Percent (2) | Prison (3) | Number (4) | Percent (5) | Number (6) | Percent (7) |
| Assault | 1927 | 36.8 | 7.3 | 896 | 42.6 | 746 | 33.0 |
| Petty assault | 1065 | 20.3 | 0.2 | 486 | 23.1 | 344 | 15.2 |
| Menace | 722 | 13.8 | 9.5 | 296 | 14.1 | 340 | 15.0 |
| Negligent bodily injury | 705 | 13.5 | 0.6 | 105 | 5.0 | 520 | 20.3 |
| Others | 817 | 15.6 | | 322 | 15.3 | 311 | 13.8 |
| Observations | 5236 | | | 2105 | | 2261 | |

*Notes*: The table reports the types of between-colleague crimes for the full sample, the male-female violence sample, and the male-male violence sample. Column (3) reports the percent sent to prison within each crime code as a proxy for severity of crimes. These numbers are calculated from the court data (not the police data). We calculate the share of all court cases within each crime code that are sent to prison. In Table 2 we collapse the data to the individual level within year, which is the level at which we observe individual characteristics, i.e. if an individual commits multiple workplace crimes in the year he is only included once. Thus, Table 2 has fewer observations than this table, since in this table multiple crimes in the same year are all included as separate observations.

**Individual Characteristics** Table 2 shows that approximately half of victims are women. In contrast, 83% of perpetrators are men. Almost by construction, victims and perpetrators of colleague violence have significantly greater labor market attachment than those involved in non-colleague violence: over 91% are working the year before the incident.[14] Table 2 reveals some key differences in the characteristics of workplace violence involving a male perpetrator and female victim (hereafter male-female violence) compared to those involving a male perpetrator and male victim (hereafter male-male violence) that go beyond simply victim gender. On average, victims and perpetrators of male-male workplace violence are the same age and have similar incomes. Male-male crimes are also slightly more likely to have multiple perpetrators compared with male-female crimes, where almost all crimes consist of a single perpetrator.

In contrast, victims of male-female workplace violence are younger and earn €11,807 less

[14] Appendix Table B.1 gives the statistics for victims and perpetrators of non-colleague assault for comparison: their employment rates are below 50%, earnings vary between €11k and €15k, and there is a substantially higher share of high-school dropouts (41-48%).

**Table 2:** Sample Means for Perpetrators and Victims

| | All | | Male-Female | | Male-Male | |
|---|---|---|---|---|---|---|
| | Perpetrator (1) | Victim (2) | Perpetrator (3) | Victim (4) | Perpetrator (5) | Victim (6) |
| Gender | 0.83 | 0.52 | 1.00 | 0.00 | 1.00 | 1.00 |
| Age | 38.78 | 37.10 | 40.35 | 37.17 | 37.51 | 36.44 |
| Share college | 0.15 | 0.13 | 0.15 | 0.16 | 0.14 | 0.09 |
| Share high school | 0.55 | 0.59 | 0.56 | 0.60 | 0.56 | 0.59 |
| Share dropouts | 0.29 | 0.29 | 0.29 | 0.25 | 0.30 | 0.33 |
| Employment | 0.94 | 0.92 | 0.94 | 0.91 | 0.95 | 0.93 |
| Income | 36598 | 29518 | 36612 | 24805 | 38950 | 33925 |
| Positive earnings | 37657 | 30642 | 37856 | 26172 | 39744 | 34796 |
| Share manager | 0.12 | 0.06 | 0.14 | 0.05 | 0.11 | 0.06 |
| Prior crimes | 2.13 | 1.39 | 2.38 | 0.82 | 2.24 | 1.76 |
| Observations | 5189 | 4962 | 2070 | 2049 | 2253 | 2077 |

*Notes*: Table reports sample means for all perpetrators and victims of colleague violence in columns (1) and (2) and then separately for male-female and male-male crimes. Data is from the police reports linked to FLEED register data. Note that in some cases there are multiple perpetrators (it is also possible but less common to have multiple victims attached to a crime code) which is why the number of perpetrators and victims are not the same. For this table, we collapse data to the individual-year level, meaning that an individual who commits multiple workplace crimes in the same year only appears once, explaining the smaller number of observations compared with Table 1.

than their male perpetrators, equivalent to 32% lower incomes. On average, the income gap between female victims and male perpetrators is approximately twice as large as the average gender pay gap within the firm (see Table 3). Additionally, male perpetrators of male-female workplace violence are approximately three times as likely to be in management using the (coarse) occupational codes compared with their female victims. Thus, victims of male-female violence are relatively low-earning women within the firm compared to their perpetrators who are relatively high-earning men. This is not the case for male-male workplace crimes, where male-male violence is more likely to occur between relative equals within the firm.

The types of crimes that characterize male-female workplace violence are also more serious than those for male-male workplace violence. Table 1 shows that 66% of male-female incidents are assaults compared to 48% of male-male incidents. Beyond assault, the next most prevalent crime for male-male workplace violence is "negligent bodily injury", which is characterised by a lack of care, while for male-female violence it is "menace", which requires a perpetrator intentionally causing fear of serious injury or death. Column (3) gives the average proportion of all perpetrators in the court data (and not just workplace violence cases) who receive a prison sentence for a given crime type to give a quantitative measure of crime severity. This measure also demonstrates that male-female crimes are relatively more severe: a much higher share of menace and assault cases in court result in prison sentences (9.5% and 7.3% respectively), compared to negligent bodily injury (0.6%). Note that Table 1 reports statistics at the case level, and thus has more observations than Table 2 which collapses the data to the individual level within year, i.e. if an individual commits multiple workplace crimes in the year, they are only included once in Table 2.

Given these differences, we largely analyze male-female violence and male-male violence separately in the rest of the paper. The economic relationship between victim and perpetrator, and the severity of the crime reported, indicate that these are likely to have very different impacts on victims and the wider firm. We also note that for the remainder of the paper we do not single out cases with a female perpetrator for two reasons. First, women are rarely perpetrators. In only 17% of cases do we observe a female perpetrator of workplace violence. Further, in many cases involving a female perpetrator, she is also recorded as a victim, meaning these are often not clear-cut cases of female perpetrated violence (see Appendix Figure C.1). There are fewer than 250 clear-cut cases of female-male violence and female-female violence. In contrast, the vast majority of male-female and male-male cases have a clear-cut perpetrator who is not also classified as a victim in the same incident. Thus, small sample sizes would make it difficult to say anything conclusive about female perpetrated crimes.

**Firm Characteristics** Turning to firm characteristics, Table 3 shows that firms in which colleague violence is reported do not appear to be negatively selected relative to all other firms in Finland. They have higher wages and higher tenure. This table also shows that firms where male-female crimes take place are not obviously negatively selected for female employees. They have a higher female share overall, a higher share of female new hires, and smaller gender pay gaps compared to male-male violent firms, and approximately the same female share and gender pay gaps as all other firms. However, it is worth noting that male-female violence occurring in firms with a reasonable share of women may be partly mechanical. A firm must have at least one woman for male-female violence to take place. The dimension along which violent and non-violent firms differ the most is firm size, as violent firms are significantly larger. This is partly mechanical: in larger firms, there are more potential combinations of colleagues between whom violence can occur.

In Appendix Table B.2 we report estimates from a simple linear probability model with a dummy for workplace violence as the outcome in column (1), and also the occurrence of male-female and male-male violence as separate outcomes in columns (2) and (3). Firms in the public sector, administration, and manufacturing are all significantly more likely to experience both male-male and male-female workplace violence relative to other industries (Appendix Figure C.2). Mining and Quarry industries are more likely to experience male-male violence, but not more likely to experience male-female violence. Controlling for industry fixed effects, we see that firms in which colleague violence occurs are slightly younger, less educated, and lower paying although the magnitudes of these coefficients are all small. There is no statistically significant difference in turnover rates nor in gender pay gaps between violent and non-violent firms.

**Table 3:** Firm Summary Statistics

| | Violent Firms | | | Other Firms |
|---|---|---|---|---|
| | All | Male-Female | Male-Male | |
| | (1) | (2) | (3) | (4) |
| **Panel A: Characteristics of the Workforce** | | | | |
| Median number workers | 36 | 29 | 44 | 4 |
| Average age of workers | 39.56 | 39.64 | 39.57 | 41.03 |
| Average wages | 28929 | 26803 | 32035 | 25603 |
| Share college | 0.13 | 0.14 | 0.11 | 0.14 |
| Share high school | 0.57 | 0.57 | 0.59 | 0.56 |
| Share dropouts | 0.30 | 0.29 | 0.31 | 0.29 |
| Average tenure | 7.38 | 7.11 | 8.06 | 7.10 |
| Turnover rate | 0.28 | 0.28 | 0.26 | 0.28 |
| Share of new hires | 0.35 | 0.36 | 0.33 | 0.31 |
| **Panel B: Gender Characteristics of the Firm** | | | | |
| Share of women | 0.38 | 0.47 | 0.21 | 0.44 |
| Female turnover rate | 0.12 | 0.14 | 0.07 | 0.14 |
| Share of female new hires | 0.14 | 0.18 | 0.08 | 0.15 |
| Average gender pay gap (male-female) | 6821 | 6768 | 7034 | 6795 |
| Median gender pay gap (male-female) | 5649 | 5385 | 6285 | 4469 |
| Share female managers | 0.24 | 0.28 | 0.15 | 0.30 |
| Observations | 4013 | 1909 | 1687 | 2631721 |

*Notes*: Table reports sample means (unless otherwise indicated) for all firms that experience between colleague violence (column 1), as well as firms that experience male-female violence (column 2) and male-male violence( column 3). Means for all other firms in Finland where between colleague violence does not take place are reported in column (4). Data is from the police reports linked to FLEED administrative register data. For this table, we collapse to the yearly level, since this is the level at which we observe firm outcomes. We also note that the smaller number of observations compared with Table 2 is due to the fact that a single firm can have multiple cases of workplace violence in the same year, but we only enter the firm once per year for this table. See Section 2 for more details on sample construction.

## 3 Empirical Strategy

In this section, we describe our empirical approach to identify the impacts of violence between colleagues on victims, perpetrators, and the broader firm relative to the counterfactual of no violent incident occurring. This is challenging because violence by nature is difficult and unethical to randomize and the timing of an incident may be endogenous to other economic shocks that affect the outcomes of interest. For example, perpetrators might target economically vulnerable colleagues who are doing badly in the firm. In such a case, a decline in a victim's income might have occurred regardless of the violent incident. Firms that experience violence could similarly be selected on both observable and unobservable characteristics.

To address these concerns, we employ the seminal event study approach (Kleven *et al.*, 2019; Dobkin *et al.*, 2018). Our main specification estimates event studies comparing victims and perpetrators of workplace violence relative to a matched control observation, similar to the approach from Schmieder *et al.* (2022) to estimate impacts of job loss. Likewise, we estimate the impact of workplace violence on firm outcomes relative to a matched control firm. Formally, we find a victim's (and perpetrator's) nearest neighbour match on the basis of their age, education level, gender, employment and income history in the five years before the incident. We restrict to victims and perpetrators that we can follow 5 years before until 5 years after the event. For firm outcomes, we find the nearest neighbor match on firm characteristics: firm size, turnover rate, industry, average age of workers, average education of workers, share of new hires, and gender composition.[15] These exercises leave us with a match for the victim, a match for the perpetrator, and a match for the firm that all appear identical on observables in the five years prior to the year in which violence occurs, but who do not experience workplace violence.

With our matched control and treatment observations in hand, we estimate the following event study:

$$Y_{ibt} = \alpha_{ib} + \sum_{j=-5, j\neq -1}^{5} \delta_j D_{b,t-j} + \pi_b + \gamma_t + \gamma_j + Age_{ib} * \gamma_j + \epsilon_{ibt}, \tag{1}$$

where $Y_{ibt}$ represents the outcome of interest for victim (perpetrator) $i$ in base-year sample $b$ at time $t$. We primarily focus on employment outcomes, although in the appendix we also examine impacts on income. For firms, $Y_{ibt}$ represents a variety of firm level outcomes, such as headcount, exit, and workforce composition, for firm $i$ in base-year sample $b$ at time $t$.

$D_{b,t-j}$ is an indicator variable for the treatment (workplace violence) separately for each year

[15] We find similar results for firm outcomes whether we use the individual match or the firm match when estimating the event studies.

$j$ since the event. $\delta_j$ are the coefficients of interest, identifying the effects of the violent incident on victim or firm outcomes relative to the matched counterfactual. We omit the year prior to the event ($j = -1$), which means that all estimates of $\delta_j$ are relative to the year before the incident. $t$ indicates the year in which violence occurred. Additionally we include individual-incident-year fixed effects (or firm-incident-year fixed effects when examining firm outcomes), $\alpha_{ib}$, year fixed effects, $\gamma_t$, time since event fixed effects, $\gamma_j$, and age at the time of incident by time since event interactions, $Age_{ib}$.[16]

In our main results we either report the separate yearly effects $\delta_j$ for the 5 years after the incident or report the difference-in-difference (DiD) estimates for our outcomes of interest. DiD estimates provide the differences in outcomes five years after versus five years before for victim, perpetrator, or firm outcomes relative to their matched counterfactual. Comparisons always occur between treated and never-treated individuals to address concerns of bias in event-study estimates (Sun and Abraham, 2020; Goodman-Bacon, 2018), i.e. this is a stacked DiD exercise as in Cengiz *et al.* (2019).

The key identifying assumption for victims and perpetrators is that the employment of the victim (perpetrator) would have evolved similarly as their matched control in the post period had there not been a violent incident. A similar assumption must hold for firms and their matched controls. In Appendix Figures C.3 and C.4 we show raw employment and income before and after the violent incident for victims and perpetrators. Prior to the violent incident the raw income and employment for both the victims and their matches, as well as the perpetrators and their matches, are identical. There is a sharp discontinuity in the labor market outcomes of both the perpetrators and the victims following a violent incident, with employment and income dropping substantially after workplace violence. We do not see these effects for the control observations, aside from a small mechanical drop in employment due to natural separation (we have required employment in a firm in year 0). Importantly, there does not exist an "Ashenfelter dip", i.e. a drop in income or employment prior to the incident for either victims or perpetrators, which might have suggested that violence was occurring in reaction to negative income trends preceding the incident. Thus,

[16]Individuals are different ages at the base year and thus this is not collinear with individual and time since event fixed effects.

these descriptive figures suggest potentially large effects on victim and perpetrator employment and income relative to their counterfactual.

There are three main concerns with identification. First, we might be worried about over-fitting on the pre-trends that could make the victim and matched control appear more similar than they actually are. To address this, we match on outcomes in only three of the five years preceding violence and re-estimate our main effects. In this analysis, we see that the pre-trends are still statistically indistinguishable in the estimated event studies, and that the estimated impacts of violence are identical.

Second, victims may be targeted and perpetrators turn violent because of low income growth prior to the event, or we could be identifying some sort of reversion to the mean. To test for this, we run a placebo specification where we redo our entire analysis, including the matching, but estimating impacts on the victim and perpetrator 5 years *prior* to the violent event. If our identification approach is valid, we would expect this exercise to function as a placebo, with no impacts on victims or perpetrators. This is precisely what we find.

Finally, victims and perpetrators might be different on a host of unobserved characteristics that influence the impact of violence on their outcomes. To test for the potential significance of this, we take future victims and perpetrators of workplace violence as the counterfactual controls. This is not our main specification because it is likely to overstate the effects of workplace violence given that, by construction, future victims and perpetrators will eventually enter employment for them to assault, or be assaulted, by a colleague. We describe this and the rest of the robustness exercises described above in more detail in Section 4.3.

### 3.1 Alternative Counterfactual

Our preferred counterfactual is one in which no violent event occurs: ideally, people could simply go to work and never be attacked by a colleague. However, one might also be interested in the impact of being attacked by a colleague from work compared to the impact of being a victim of a similar crime, but when the perpetrator is not a colleague. Similarly for perpetrators. For firms, we might be interested in whether there is a difference in outcomes when an employed perpetrator attacks a colleagues versus if they commit a crime against a non-colleague. At first glance, this seems like an equally valid counterfactual and could enable us to identify whether there is anything uniquely special about violence between colleagues versus violence in general.

In Sections 4 and 5, we discuss results for this alternative "violent" counterfactual, restricting our analysis to the four most frequent crime codes (Table 1). For individuals, we identify their matched controls from the set of victims and perpetrators of these crimes who were not attacking or attacked by colleagues. We match exactly on employment status in the year preceding the crime and on crime type and then identify an individual's nearest neighbour on the basis of their age, education, and employment/income outcomes in the five years preceding an incident. For firms, we identify matched controls from the set of firms who employed a perpetrator but who committed an offence against a non-colleague. This violent counterfactual will not comprise our main specification. We prefer our main specification because it (a) captures the full repercussions of an assault by a colleague, not just the impact relative to other types of victimization and (b) captures the policy relevant counterfactual, namely being able to go to work without being assaulted.

# 4 Impacts on Victims and Perpetrators

## 4.1 Main Results

Figure 1 gives the event study coefficients of interest from Equation 1 for victims and perpetrators with employment as the outcome of interest. Figure 2 reports the aggregated difference-in-differences results.[17]

In the first row of Figure 1 we see that colleague violence leads to immediate, large, and persistent drops in employment for both victims and perpetrators.[18] In the second row, we see that the employment rate of victims of male-female violence falls 4.6 percentage points in the first year after the incident relative to their matched controls, which grows to a 10 percentage point fall in

[17]Table B.3 gives the aggregated difference-in-differences results and results from various robustness exercises.

[18]Appendix Figure C.5 gives the equivalent results for percent income losses.

employment by five years later.[19] Perpetrators of male-female workplace violence also experience negative impacts to their employment.[20] The last row of Figure 1 reports event-study coefficients for victims and perpetrators of male-male violence. Victims see an immediate 2.3 percentage point drop in employment that grows to 7.1 percentage points 5 years later. Perpetrators see an immediate employment decline of 10 percentage points which grows to 11.5 percentage points five years later.

The event studies not only show that workplace violence has large economic consequences for victims and perpetrators. They also demonstrate a striking asymmetry in the labor market impacts across male-female and male-male violence. Figure 2 succinctly summarizes this, showing the overall DiD impacts on the perpetrators and victims of male-female versus male-male crimes. For male-male violence, perpetrators suffer a significantly greater labor market cost than their victims: their employment rates fall by over 10 percentage points in the five years following an incident, and this impact is significantly different than the 4.2 percentage point decline in employment for victims. This is the result one might expect. It seems reasonable and appropriate that perpetrators of workplace violence experience more negative outcomes after attacking a colleague than their victims. In particular, we might expect the firm to punish perpetrators after the incident, including possibly firing the perpetrator.

In contrast, there is no statistically significant difference in the employment impacts of male-female violence across perpetrators and victims. Indeed, if anything, perpetrators experience smaller labor market consequences compared to their victims. Overall, perpetrators' employment rates fall by 5.2 percentage points. This is smaller than, although not statistically distinguishable from, their female victims whose employment falls 8.4 percentage points. We see a similar asymmetry in the impact of workplace violence on the percent earnings losses of victims versus perpetrators for male-female versus male-male crimes, see Appendix Figure C.6 and

[19] Victims of male-female crimes experience a loss in income of €2,198 on average (see Appendix Table B.4). This is a sizable magnitude: column (3) of Table B.4 shows that female victims' incomes fall 16% on average compared to the pre-violence baseline. Note that the slight increase in employment at time zero is mechanical, due to our requiring employment at the time of the incident in either year -1 or year 0 and the fact that we do not match the counterfactual control in year zero (see Section 2 for details).

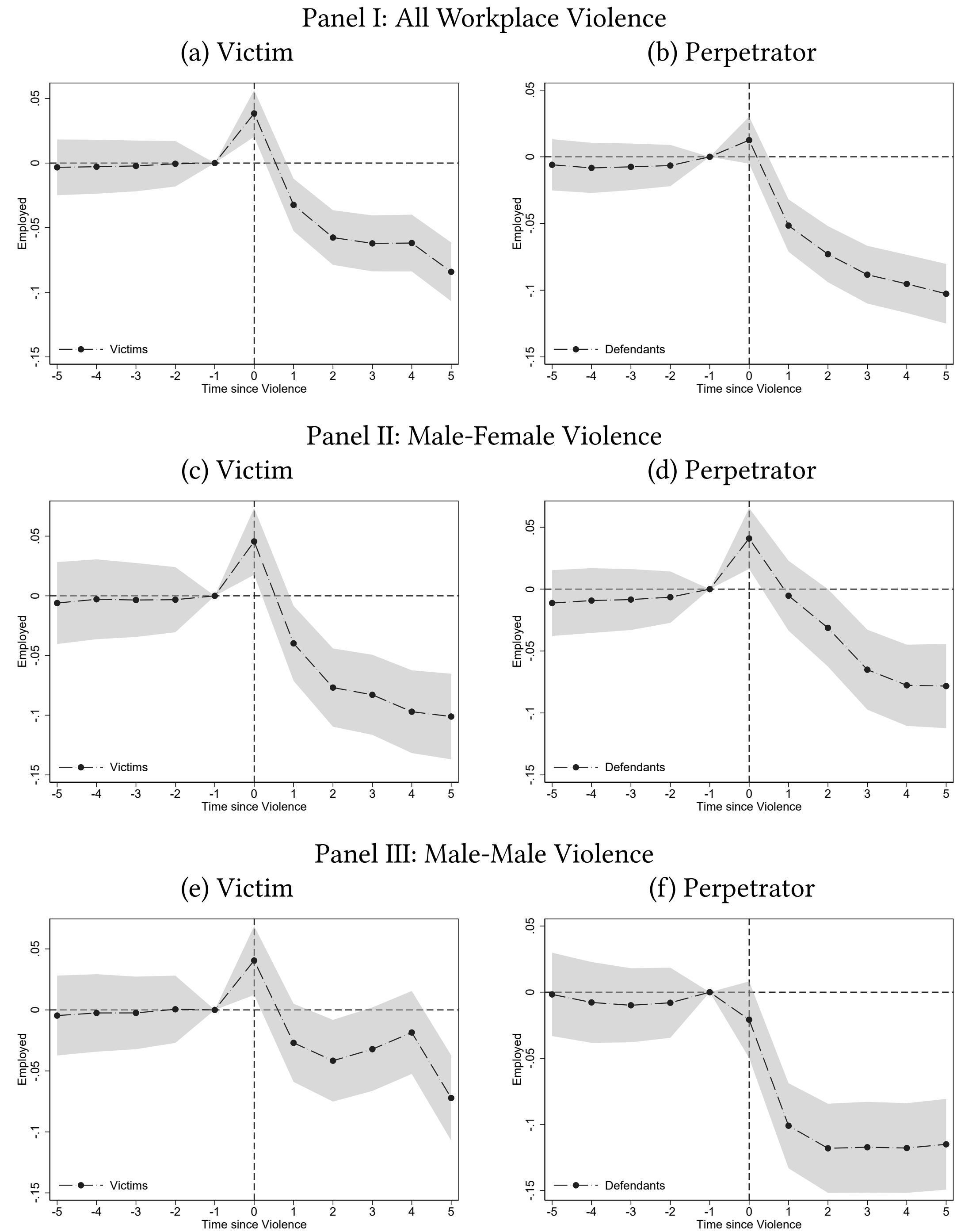
[20] In the five years after the incident, there is a 5.2 percentage point drop in perpetrator's employment and an 8% fall in income relative to the pre-violence baseline (see Table B.3).

column (3) of Appendix Table B.4.

In Appendix Figure C.7 we examine the impact of a violent incident on leaving the firm. Leaving the firm can occur if the victim/perpetrator moves to unemployment, but also if they leave for a different firm but remain in employment. We find the same asymmetry in the perpetrator impacts of workplace violence: there is no difference between perpetrators of male-female violence and their matched control in whether they remain in the same firm. However, male-male perpetrators are 8 percentage points less likely to be employed in the same firm.[21] Victims of male-female violence are, consistent with our employment effects, less likely to remain in the same firm, although the difference with male-male victims is not statistically significant.

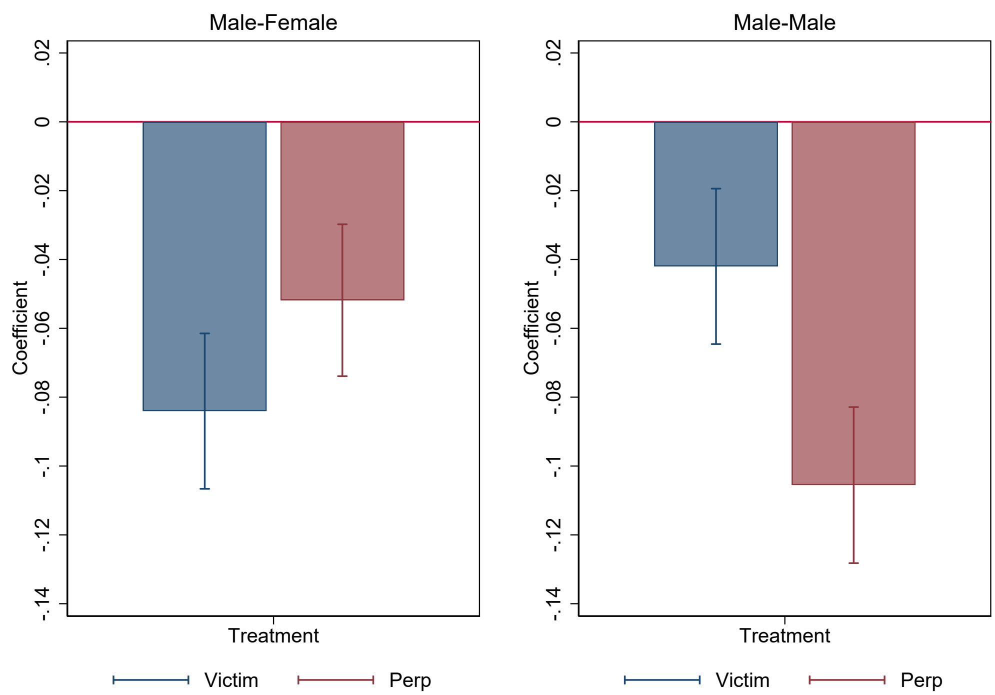
[21]This is consistent with perpetrators who are not fired being *more* likely to remain at the same firm relative to their matched counterfactual. Thus it appears that conditional on not being let go and moving into unemployment, perpetrators of violence against colleagues are more likely to remain in the firm following the incident, and this is especially true for perpetrators of male-female violence.

**Figure 1:** Impact of Workplace Violence on Employment of Victims and Perpetrators




*Notes* Each figure reports the impact of a violent incident between colleagues that results in a police report on employment of the victim (left-hand side) or perpetrator (right-hand side). First row reports effects for all workplace crimes. Second row reports results for male-female violence. Third row reports effects for male-male violence. The estimates use the matched control to identify effects 5 years before and 5 years after a violent incident against a colleague (see equation 1). Employment is measured at the end of the year.

**Figure 2:** Asymmetry in Employment Impacts of Workplace Violence




*Notes:* Figure reports estimates of $\delta_t$ obtained using Equation (1) where we collapse into a pre- and post-period to recover difference-in-difference estimates. Left-hand figure reports DiD estimates for male-female violence for victims (in the blue bar on the left) and perpetrators (in the red bar on the right). Similarly for male-male violence in the right-hand figure. 95% confidence intervals depicted in whiskers around the estimates. Employment is measured at the end of the year.

The limited employment losses for perpetrators of male-female workplace violence is also evident when one considers the counterfactual of violence taking place between non-colleagues. Recall that for this analysis we identify our matched control from the set of victims and perpetrators who are not colleagues but who also experience one of the four most common crime types recorded in Table 1.[22] Appendix Figure C.8 gives the difference-in-differences estimates for victims and perpetrators of male-female and male-male violence. For male-male violence, both victims and perpetrators suffer 4.5 percentage point larger employment losses than observationally equivalent individuals who experience or commit the same category of crime with a non-colleague. For male-female violence, however, while victims of colleague and non-colleague

[22]We match exactly on crime code and employment status in the year of and before the event, and identify their nearest neighbor on the basis of the same characteristics in our main specification.

violence suffer identical labor market impacts, perpetrators of colleague violence suffer significantly smaller employment losses. Perpetrators employment rates are 3.1 percentage points greater over the five years following the incident compared to an observationally equivalent individual who commits a crime against a non-colleague (p-value=0.047). This is a striking finding given that violence against a colleague is likely more public to decision-makers within the firm than non-colleague violence and these decision-makers could naturally be expected to fire the perpetrator in order to preserve workplace culture.

Perhaps the limited employment impacts on perpetrators of male-female violence compared to perpetrators of male-male violence are explained by differences in the observed characteristics of the crimes committed or of the victims and perpetrators themselves. As described in Section 2.2, cases of male-female violence are more likely to be assaults and victims are, for example, lower earners in the firm compared to victims of male-male crime. A priori, one might expect the impacts on perpetrators to be larger for male-female violence given that they commit more serious crimes. However, perhaps the other individual characteristics reverse this expected result and explain the relatively smaller employment consequences for perpetrators of male-female violence.

To assess whether these observable differences drive the asymmetry in employment effects across male-female and male-male violence, we pool observations for male-female and male-male crimes and estimate interacted difference-in-differences specifications separately for victims and perpetrators. We interact the treatment indicator with an individual's education, age, prior earnings ($\mathbf{X}_{il}$), whether the crime is an assault, and an indicator for the gender of the victim. Formally we estimate:

$$Y_{ibt} = \delta D_{ibt} + \beta D_{ibt} \times \text{FemaleVictim}_i + \sum_l \eta_l D_{ibt} \times X_{il} + \gamma_t + \alpha_{ib} + \epsilon_{ibt} \tag{2}$$

where $Y_{ibt}$ is an employment dummy for the victim (perpetrator) $i$ in base-year sample $b$ at time $t$, $D$ is a treatment indicator equal to 1 if an individual is a victim or perpetrator of workplace violence after the incident occurs. We again include individual-incident-year fixed effects, $\alpha_{ib}$, and year fixed effects, $\gamma_t$.

Appendix Figure C.9 gives the coefficient $\beta$ that captures differences in the employment effects by the gender of the victim. For victims, we see that this coefficient is a precise zero: there are no statistically significant differences in the outcomes for male and female victims after controlling for differences in their non-gender characteristics and the crimes committed. Indeed, even without interacting this treatment dummy with observable characteristics, there is no statistically significant gender difference in victim impacts in the pooled regression (p-value= 0.995).[23] However, even with our rich set of controls, perpetrators of male-female violence still face significantly smaller employment consequences than those of male-male violence (p-value= 0.055).

### 4.2 The Role of Power Discrepancies Between Victim and Perpetrator

In this section we explore another possible explanation for the asymmetries documented in Figure 2: the economic standing of perpetrators relative to victims within the firm. Anecdotal evidence from the #MeToo movement suggests that inequality between victims and perpetrators, and being attacked by an individual in a position of power, is especially problematic. When describing the aftermath of her assault at the hands of Harvey Weinstein, Rowena Chiu wrote *"Harvey was a power player, and I was the lowest person on the totem pole. Assistants are the unseen work force that props Hollywood up, and yet we have zero leverage. I was invisible and inconsequential."* (Chiu, 2019).[24] This was a common theme for #MeToo victims, even when the perpetrators were not as famous and powerful as Harvey Weinstein. For example, a Starbucks worker commented to the Huffington Post *"I worked at Starbucks for three years during College. When one of my coworkers and I reported our shift manager for sexual harassment we were told we'd be laid off the following week"* (Agrawal, 2017). Thus, it seems that having perpetrators in a position of power might play an important role in how these events are adjudicated and the eventual impacts on victim and perpetrator outcomes.

Motivated by this, in Table 4 we consider heterogeneity in the impacts of workplace violence

[23] Note that in the pooled regressions, age-time fixed effects are controlled for as per our baseline specification.

[24] Widespread media coverage of these events did eventually lead to repercussions in some cases. Most notably, Harvey Weinstein was convicted and sentenced to prison in 2020 after a 5 day deliberation. The first police report against Weinstein occurred in 2015 and was quickly dismissed after a 2 week investigation.

by the economic standing of victims relative to perpetrators within the firm.[25] First, we interact the "treatment", i.e. an attack by a colleague, with whether the perpetrator is a manager for male-female crimes in column (1) and for male-male crimes in column (3) of Table 4. This exercise uses the coarse occupation variable in the data, and whether this variable indicates a managerial role for the perpetrator. Second, we interact the treatment of workplace violence with the difference in the income rank within the firm of the perpetrator and their victim, a broader measure of economic power differences between victim and perpetrator. This approach is possible because we observe the universe of workers in the firm and their labor market earnings, in addition to the perpetrator and his victim. For example, the income rank gap between a perpetrator at the 75th income percentile within the firm and a victim at the 25th income percentile would be 0.5. We report results of this second exercise in columns (2) and (4) of Table 4.

Table 4 shows that the economic power of perpetrators matters for the consequences of both male-female and male-male workplace violence. For both types of crime, the employment impact on victims is greater when perpetrators are managers and there is a greater income gap between the victim and perpetrator. For perpetrators, the effect is the opposite: their employment rates are less severely impacted when they occupy relative positions of power within the firm. For male-female (male-male) crime, victims' employment rates fall by 5.6 (7.4) percentage points *more* when their perpetrator is a manager. However, perpetrators who are managers are 5.9 (13.3) percentage points *less* likely to be unemployed in the five years following an incident. Thus, power within the firm allows perpetrators to avoid more severe consequences, while their victims experience worse outcomes.

These results do not admit an interpretation that female victims are simply more harmed by and/or less effective at holding powerful perpetrators to account than male victims are: there are insignificant differences in the treatment effect of workplace violence between male and female victims. However, these results could partially account for the limited employment impacts on male perpetrators of male-female crimes where such economic inequality between the victim and perpetrator is more common (see Section 2.2).

[25] In Appendix Table B.5, we consider heterogeneity just by individual characteristics rather than differences across victims and perpetrators. Few significant patterns emerge.

**Table 4:** The Role of Power Discrepancies between Victim and Perpetrator

| Dependant Variable: | Victim Employment | | Perpetrator Employment | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel A: Male-Female** | | | | |
| Treatment*Perp is Manager | -0.056 | | 0.059 | |
| | (0.029) | | (0.018) | |
| Treatment*Income Gap | | -0.018 | | 0.065 |
| | | (0.017) | | (0.017) |
| Treatment | -0.079 | -0.075 | -0.058 | -0.085 |
| | (0.012) | (0.014) | (0.012) | (0.016) |
| Observations | 29,813 | 29,813 | 30,056 | 30,056 |
| Dependant variable mean | 0.824 | 0.824 | 0.845 | 0.845 |
| **Panel B: Male-Male** | | | | |
| Treatment*Perp is Manager | -0.074 | | 0.133 | |
| | (0.039) | | (0.022) | |
| Treatment*Income Gap | | -0.059 | | 0.116 |
| | | (0.019) | | (0.017) |
| Treatment | -0.036 | -0.019 | -0.117 | -0.152 |
| | (0.012) | (0.013) | (0.012) | (0.015) |
| Observations | 27,618 | 27,618 | 28,046 | 28,046 |
| Dependant variable mean | 0.819 | 0.819 | 0.828 | 0.828 |
| Year fixed effects | ✓ | ✓ | ✓ | ✓ |
| Time since crime fixed effects | ✓ | ✓ | ✓ | ✓ |
| Individual fixed effects | ✓ | ✓ | ✓ | ✓ |
| Age x time since crime | ✓ | ✓ | ✓ | ✓ |

*Notes*: Table reports difference-in-difference estimates from Equation (1) collapsed into a pre- and post-period. In all cases the dependent variable is employment measured at the end of the year. Panel A reports estimates for only male-female workplace crimes while Panel B report estimates for male-male workplace crimes. Data is from police reports linked to FLEED register data. Manager is defined from the occupation variable in the data. Income gap is the difference in the income rank within the firm of the perpetrator and their victim.

To formally test the degree to which economic power imbalances can explain the gaps in perpetrator outcomes, we interact the treatment of workplace violence with the income and managerial gap between victims and perpetrators in Equation 2. Appendix Figure C.9 shows that the coefficient on the interaction with victim gender remains insignificant and reduces in magnitude. For perpetrators the difference in employment effects for male-female versus male-male workplace violence also reduces in magnitude and becomes statistically insignificant once we control for the power imbalance between victim and perpetrator, albeit imprecisely estimated (p-value = 0.227). Thus, differences in the perpetrator employment effects of male-female versus male-male crimes in Figure 2 are partly due to a composition effect, i.e. power imbalances are much more common for male-female crimes than for male-male ones, and power imbalances play a key role in the impacts of workplace violence.

### 4.3 Robustness

In this section, we probe the robustness of our results to address the potential identification challenges described in Section 3. However, we first note that the immediate, large, and discontinuous changes in victims and perpetrator labor market outcomes at the point of a violent incident relative to their own pre-violence outcomes and those of their matched controls, and the fact that these differences persists for at least 5 years after the incident, rules out many alternative explanations.

One might be concerned about over-fitting given the richness of our matching. In Table B.3 we report estimates that are robust to dropping two of the five pre-periods when identifying a nearest-neighbor match. The over-fitting robustness results are identical to our main results for both victims and perpetrators. In Appendix Figures C.10 and C.12 we show that the full event study estimates also look the same. In particular, pre-trends remain flat.

Another way to address the concern that our matching approach simply over-fits in the pre-period, leading to natural reversion to the mean in the post period, is to estimate the effects of a placebo event. To implement this approach, we artificially move our event to five years before the violent incident. Running our estimation strategy but estimating effects 5 years prior to workplace violence, when no crime occurs, should return a null-effect. This approach also addresses the concern that victims might be targeted because of their low income growth potential or that victims are different in some other unobservable way that is correlated with both being the victim of violence and the drops in employment we have documented.

In order to conduct this analysis, we repeat our matching procedure in the years before the new event. This is because, by construction, the nearest neighbor matches identified in our main specification were chosen to be observationally equivalent in the five years before violence, i.e. the post period in the placebo test. When we re-match, we only match 3 years prior to the placebo event year to avoid dropping many observations: ten years before a violent incident, a nontrivial share of the observations are in school rather than the workforce given the average ages of our sample and that individuals exit schooling in Nordic countries at later ages. Further, we restrict the set of potential matches to those who are employed at the point at which (future) violence occurs. Otherwise there would be a mechanical positive effect on employment rates of victims and perpetrators of workplace violence since they are, by construction, employed at the time of the future realized crime. In Appendix Figures C.11 and C.13 we find that there is no significant impact on victim nor perpetrator employment using this placebo, and we report the overall DiD estimates from this exercise in Table B.3.

Victims might still be different in some other dimension not captured in the matching and individual fixed effects in our main results, not apparent in the pre-trends in the main analysis or the over-fitting robustness check, and in a way that also does not show up in the placebo exercise above. To address this possibility, we can use a completely different control group without implementing matching. Specifically, we use those who will go on to be victims of a crime by a colleague, but at a much later date (i.e. more than 5 years post the event, so that we are still comparing treated and never treated within the estimation window). The underlying assumption with this alternative approach is that those who are victims of violence are similar, and so we can compare them to each other and use the quasi-randomness of the timing of the violent incident to identify effects. Similarly for perpetrators.

We present results using this alternative approach to constructing the counterfactual in the third row of Appendix Figure C.10 for victims (third row of Appendix Figure C.12 for perpetrators). For income, the pre-trends look flat, while there is a slight upward slope for employment. This lack of major pre-trends is particularly striking because we are not matching on the pre-period in this robustness exercise. The negative effects for both employment and income following the violent incident are significantly larger using this alternative specification. However, this is to be expected: victims in the future, when used earlier in time as a control, will be younger and so likely experiencing more earnings growth. Moreover, by definition they will be employed in the future, so we expect future employment to be higher for this control group. This is why we do not use this as our main specification, as we believe that these results likely overstate the true effect size. These results suggest that our main outcomes are potentially a conservative estimate of the impacts of violent incidents between colleagues on victims.

Finally, as discussed in Section 2, the violent events we identify do not necessarily occur within the firm while at work. We therefore check the robustness of our results to excluding incidents where victim and perpetrators were cohabiting at the time of the crime or the year before. There are no cases of male-male violence where individuals were cohabiting at the time of the event, so this analysis focuses on male-female violence. Appendix Figure C.14 shows that there is no statistically significant difference in our results when excluding intimate partner cases. The point estimates for perpetrators are indistinguishable, and the victim effects are larger, if not significantly so, when we exclude observations from victims and perpetrators who were cohabiting. Our results are thus not driven by domestic violence cases.

## 5 Impacts on the Firm

Does colleague violence have impacts that extend beyond the victim and perpetrator? We first consider measures of the headcount of the firm and whether the firm remains in business (Appendix Table B.7). We find no overall impact of colleague violence on headcount nor on firm death for either male-female or male-male violence.

Even if the overall size of the workforce does not change, the composition of workers at affected firms may change if workers with systematically different characteristics leave or join following a violent incident. For example, following an incident of male-female violence in the firm, other women in the firm may be more likely to leave (especially given the low separation rates for perpetrators).[26] Alternatively, hiring rates could be affected. As the majority of perpe-

[26]Folke and Rickne (2022) show that gender minority victims of lower-level sexual harassment have higher turnover rates.

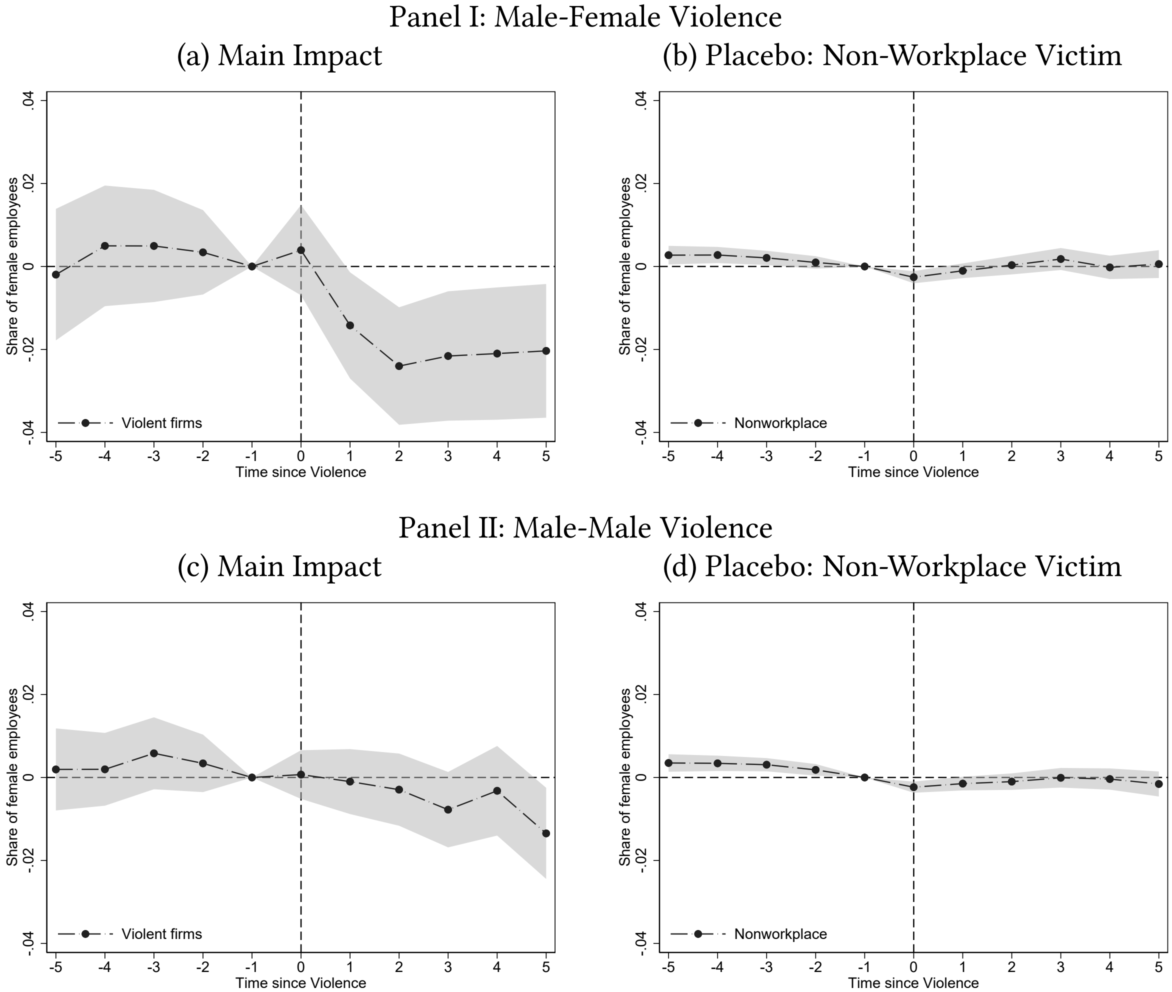
trators are men, this could lead the firm to hire fewer men if men in general, rather than just the perpetrator, are punished. Alternatively, women may be seen as creating disruption for firms following male-female violence, adding a friction to their hiring.[27] Thus, we next consider the impact of workplace violence on the share of women employed by the firm in general and not just the impact on the female (or male) victim and perpetrator.

Figure 3 Panel I (a) demonstrates that after an incident of male-female workplace violence there is a significant decline in the proportion of women employed in firms where such violence took place relative to their matched control firm. The effect is quantitatively large and persistent: relative to the pre-incident mean, the share of women employed by the firm falls by 2 percentage points by five years after the incident. Figure 3 Panel II (b) demonstrates that this effect is isolated to firms where male-female violence occurs. There is no statistically significant effect of male-male violence on the share of women employed in the firm (and equivalently, no significant impact on the share of men employed).

Perhaps this decrease in the share of women employed by the firm is not due to between colleague violence. Instead, perhaps any time the firm employs a perpetrator of a violent crime, this causes the share of women employed to decline, even when the victim is not employed by the same firm (i.e. she is employed by another firm or unemployed), because of this person's toxic presence. We explore this in a placebo test in Figure 3 Panels (b) and (d), where we estimate the impact of a violent crime where the perpetrator is employed by the firm, but the victim is not, on the share female within the firm. We find precise zeros for both male-female and male-male violence, with no impact on the broader firm from such cases. Thus, our results appear to be uniquely driven by the fact that one colleague attacked another colleague.

[27] This is a similar hypothesis to that explored in Sarsons (2017), who shows that after a female doctor experiences a patient death, referring doctors are less likely to refer cases to women *in general*, while no such reaction is apparent for male doctors who experience a patient death.

**Figure 3:** Impact on Share Female Employees in the Firm



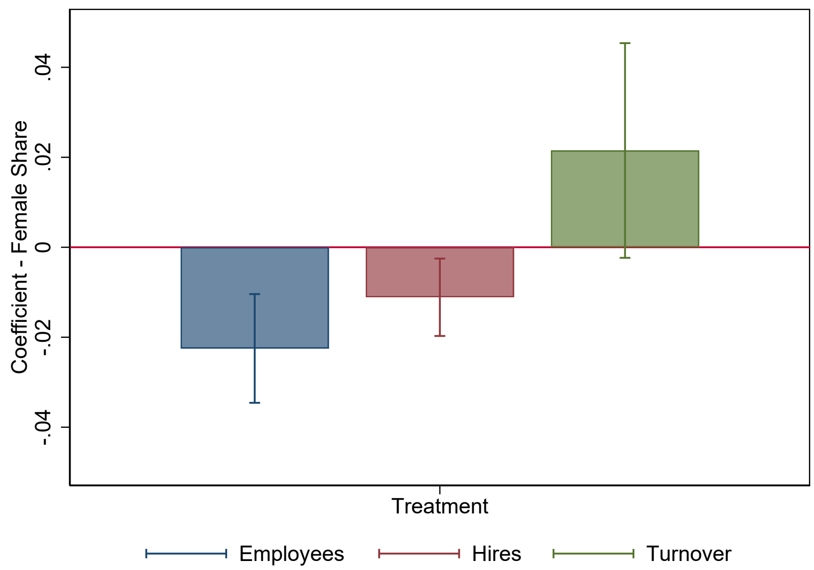

*Notes:* Left-hand-side figures show the impact of a violent incident between colleagues on the share of female workers at the firm in which both perpetrator and victim were employed at the time of the incident. The estimates use the matched control event study design described in Section 3 to identify effects 5 years before and 5 years after a violent incident against a colleague. Right-hand-side figures show the impacts of a "placebo" violent incident where the perpetrator is employed by the firm but the victim is not on the share of female employees in the firm. Horizontal axis displays time in years. Dashed vertical lines indicate the year of between-colleague violence. Panel I shows results restricting to incidents involving a male perpetrator and a female victim. Panel II shows results with male perpetrators and male victims. Sample construction and measurement described in Section 2.

Next, we explore whether the decrease in the share of women in the firm is explained by more female employees leaving the firm or by the firm hiring fewer women after the incident. We find evidence that both dimensions are relevant in Figure 4.[28] The proportion of women amongst firm leavers after male-female violence increases (p-value=0.077), while the share of

[28]See Appendix Figure C.16 for the equivalent results for male-male crimes. As expected the figure shows no significant effects on any dimensions for male-male violence.

women amongst new hires falls (p-value = 0.011). While our research design removes any time invariant differences across firms, including time invariant hiring practices, this is particularly interesting given that Table 3 shows that firms where male-female violence takes place are not firms that initially avoided hiring women. Instead, these firms had the same share of women employees and were equally likely to hire women compared with all other nonviolent firms prior to the incident.

**Figure 4:** Individual Components of the Drop in Share Female Employees for Male-Female Violence




*Notes:* Figure reports DiD estimates of the impact of between colleague violence on the overall share of women in the firm (in blue, leftmost bar), the share of women amongst new hires (in red, middle bar), and female turnover in the firm (in green, rightmost bar). Impacts shown for male-female between-colleague violence. Turnover is measured as the share of women amongst workers leaving the firm.

### 5.1 The Whisper Network and the Decline in Women Hires

Falls in the proportion of women hired by the firm could be driven by supply side factors, i.e. women being less likely to apply for jobs at the firm, or by demand side factors, i.e. the firm being less likely to hire women from a given set of applicants. We cannot observe the characteristics of applicants to the firm to analyze whether fewer women apply for positions after male-female violence. Instead, we investigate hiring from within the networks of existing employees to capture the potential influence of supply side factors. Existing employee networks is an obvious way in which potential future hires could find out about and avoid workplaces where one employee attacked another. A large literature shows that within-network hiring provides an important and high quality pool of potential applicants to the firm (Marmaros and Sacerdote, 2002; Bayer *et al.*, 2008; Beaman and Magruder, 2012; Brown *et al.*, 2016; Dustmann *et al.*, 2016). For example, Barwick *et al.* (2019) find that 38 (30) percent of workers in China (United States) find jobs through personal connections. Moreover, Hensvik and Skans (2016) show that firms are able to hire workers who are higher quality through referrals from existing highly productive workers.

Analyzing changes in network hiring also provides an assessment of the effectiveness of the "Whisper Network", a term popularized following #MeToo to define the informal dissemination of information among women. The whisper network is supposed to provide information to other women about bad firms and bad actors so that women can avoid them. "The whisper network is an informal but relatively orderly reporting method, regulated by the direct accountability of a social ecosystem: if I give you false information, then my credibility and relationships will suffer" (Tolentino, 2017). If the whisper network is effective, we would expect to see a reduction in the share of people hired from within existing employee networks following workplace violence. Given these events are recorded in police reports, but are not public record, the whisper network is the most likely way such information could circulate. Combined, the possibility of a whisper network and the importance of existing employee networks in driving hiring decisions could explain the drop in the share of female new hires we have documented.

To test whether within network hiring decreases following a violent incident, we consider two types of networks. First, we estimate the impact on the share of employees hired from previous colleagues of the firm's existing workers. To do so, we identify any person each existing employee worked with at the same time in some previous employment stint at some other firm in the 10 years prior to the incident year. Our definition of networks of past colleagues is consistent with that used in Hensvik and Skans (2016). This makes up our "Previous Colleagues" network. Second, we link to all siblings of existing employees, and estimate the impact of a violent incident on hiring within these family networks. This makes up our "Family" network.

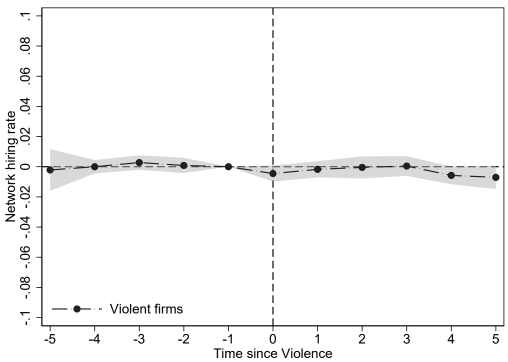
We report event study estimates of the impact of between-colleague violence on the share of new hires that come from each of these two possible networks in Figure 5. We find no impact of a violent incident between colleagues on the amount of within network hiring done by the violent firm. The figures indicate precise null results for past colleagues networks, and a slightly noisier null for family networks. However, perhaps there is simply heterogeneity across networks. "Whisper networks" are generally assumed to function primarily between women. Thus, in Panel II of Figure 5 we re-estimate the main results but instead only look at the networks of women employees. We still find no impact on the hiring of future employees from existing women employees' networks.

Together these results suggest that a supply side story, in which women are less likely to apply to violent firms, is unlikely to account for the reduction in women hired following male-female workplace violence. If those most closely connected to existing employees, and thus most likely to have knowledge of the incident, show no reaction, then it is difficult to imagine a significant response from those who are less connected and less likely to hear about the incident. For a more general supply side story to rationalize our findings, one must believe that women who are not connected to the firm by previous co-workers and family have better information about police reports and react to them by no longer applying to work at the firm. We therefore conclude that the drop in the share of female new hires is consistent with women being less likely to be hired from a given applicant pool. In other words, firms where male-female violence takes places hire fewer women following the incident.

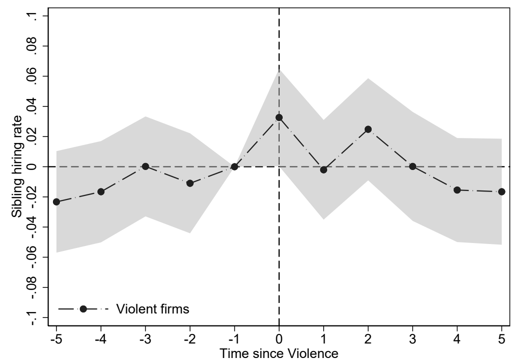
**Figure 5:** Hiring from Within Employee Networks

Panel I: All Employee Networks

(a) Previous Colleagues

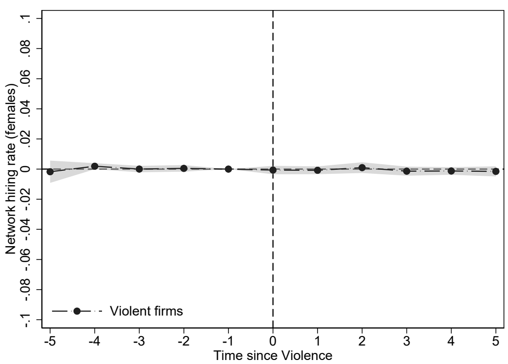



(b) Family

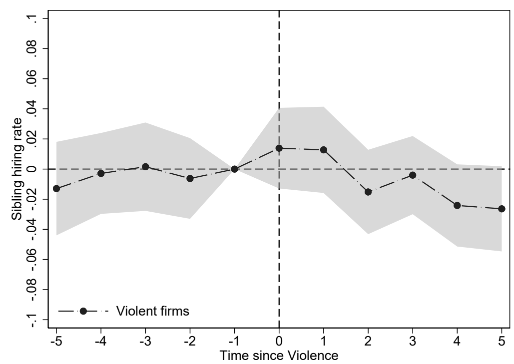



Panel II: Only Female Employee Networks

(c) Previous Colleagues




(d) Family




*Notes:* Figure shows the impact of between-colleague violence on the share of hires that come from within two possible networks of the firm's existing colleagues. First, in the left-hand-side figures we estimate the impact of workplace violence on the hiring of "Previous Colleagues", which refers to hiring past colleagues of current employees. Past colleagues consists of all current employees' past colleagues from the previous 10 years, similar to Hensvik and Skans (2016). Second, in the right-hand-side figures labeled "Family", we explore the impact of workplace violence on the hiring of siblings of existing employees. Panel I shows results for networks of all current employees, Panel II restricts estimates to only the networks of existing female employees. All estimates reflect event-study estimates of equation 1 using the matched control to identify effects 5 years before and 5 years after a violent incident against a colleague.

## 6 The Role of Managers

Firm differences in the management of colleague violence could mediate or accentuate the impact of violence on the wider workforce. Managers play an important role in determining the success of a firm (Bertrand and Schoar, 2003; Bloom *et al.*, 2007, 2013; Bandiera *et al.*, 2020; Gosnell *et al.*, 2020). Estimates suggest that differences in management account for 20% of the variation in productivity across plants (Bloom *et al.*, 2019). A smaller literature documents important heterogeneity in how male and female decision makers interpret and respond to negative (or positive) shocks, and how the gender of the individual responsible for the shock might change the response of the manager (Benson *et al.*, 2021; Chakraborty *et al.*, 2021; Sarsons, 2017).[29] Most closely related to this paper, Egan *et al.* (2022) find that following incidents of financial misconduct by financial advisers, women who commit such misconduct are more likely to be fired. They find that this disparity in the consequences for financial misconduct by gender are driven entirely by male-managed firms.

Motivated by these facts, we consider heterogeneity in the impact of male-female violence on the share of female workers in the firm by the proportion of women in decision-making positions within the firm. Following Bender *et al.* (2018), we identify workers in the top 20% of earners in the firm as those with decision-making power. If the proportion of women in the top 20% is above the median, then we label the firm female-managed. On average, women comprise 28% of the top 20% of earners in firms where male-female violence occurs, with a standard deviation of 36%. Given that there are significant cross-industry differences in the proportion of women amongst the highest earners, we control for the industry share of women in the top 20% of all earners (and its interaction with the treatment variable) in all specifications to ensure we capture firm as opposed to industry heterogeneity.

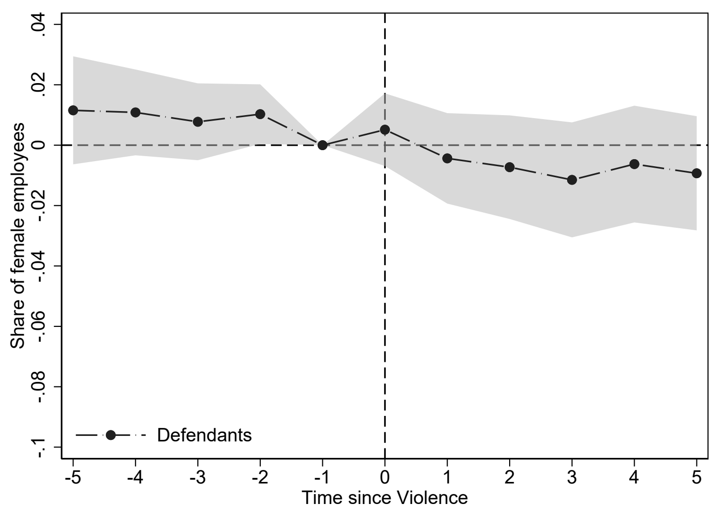
Figure 6 shows that while there is a significant decline in the share of female employees following an incident of male-female violence in male-managed firms, we see no significant impact on the share of female employees for female-managed firms. The persistence in the fall of women for male-managed firms is particularly striking, with significant negative effects lasting at least five years, compared to no significant effects and estimates close to zero for all years after the incident for female-managed firms. The overall impacts are also large, with an almost 6 percentage point decline in the share of women employed in male-managed firms by five years after male-female violence. This effect is quantitatively significant relative to the baseline share of women employed in these firms of 24.1%. In Table 6 we estimate the role of male versus female

[29] See also the literature on how female leadership impacts policy (Ford and Pande, 2011; Bertrand *et al.*, 2019).

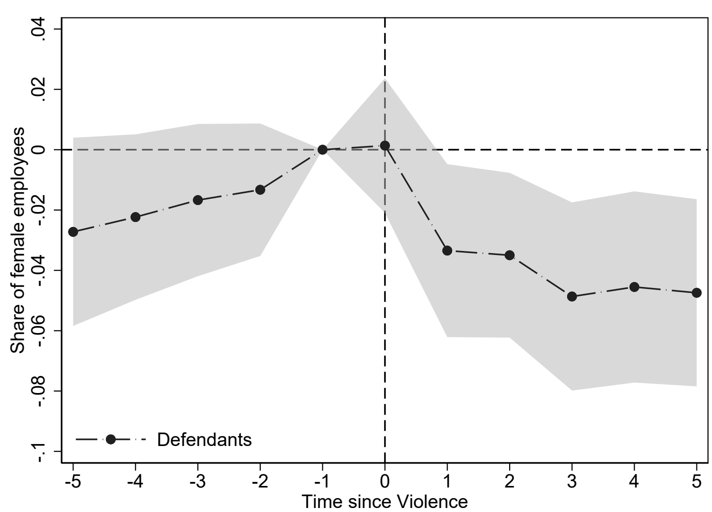
management in a regression framework. Column (1) interacts the treatment variable of workplace violence with a dummy variable indicating whether the firm is female-managed. We find that female management is associated with significantly more women in the firm following male-female workplace violence (p-value=0.011), such that the gender composition of the firm remains unchanged.

**Figure 6:** Impact on Share Female Employees By Firm Management for Male-Female Crimes

(a) Female-Managed Firms




(b) Male-Managed Firms




*Notes:* Figure shows the impact of male-female violence on the share of female workers in the firm separately for female-managed firms (top figure) versus male-managed firms (bottom figure). We define management as "male" if the share of men in the top 20% of earners is above the median share, and "female" otherwise.

### 6.1 What Do Female Managed Firms Do Differently?

How does female management mediate the impact of male-female violence on the gender composition of the firm? A surprising finding of Section 4 was that perpetrators of male-female violence face relatively limited labor market costs compared to both perpetrators of male-male colleague violence and also perpetrators of male-female non-colleague violence. We thus explore whether perpetrators are more likely to lose their job under female management and how this influences the wider repercussions of violence on the workforce. Women employees might feel more comfortable remaining in a firm where perpetrators of male-female violence face consequences for their actions. Moreover, managers who show a willingness to hold workers who perpetrate violence to account might also be less likely to punish women in general following male-female violence, i.e. they may be less likely to shift their hiring towards men following male-female violence. For example, a manager who didn't fire the perpetrator might have to worry about the perpetrator attacking future women. One way to reduce that possibility is to simply stop hiring women, as we found occurs following male-female violence (see Figure 4).

We first analyze the relationship between female management and the individual labor market impacts of violence on victims and perpetrators. Table 5 gives the difference-in-differences coefficients on the treatment variable and its interaction with a dummy variable for whether the firm is female-managed. Victim outcomes are not significantly influenced by the gender composition of management for both male and female victims. However, perpetrators have significantly lower employment rates following a violent incident in female-managed firms: for both male-female and male-male violence, perpetrators in female-managed firms have an approximately 4.5 percentage point greater reduction in employment compared to their matched control and relative to male-managed firms. Indeed, for male-female violence, the entire reduction in perpetrator employment is isolated to female-managed firms.

**Table 5:** Female Management and Impacts on Victim and Perpetrator Employment

| | Dependant Variable: Employment | | | |
|---|---|---|---|---|
| | Male-Female | | Male-Male | |
| | Victim | Perpetrator | Victim | Perpetrator |
| | (1) | (2) | (3) | (4) |
| Treatment*Female Manager | -0.018 | -0.044 | 0.016 | -0.047 |
| | (0.017) | (0.017) | (0.017) | (0.018) |
| Treatment | -0.075 | 0.030 | -0.050 | -0.082 |
| | (0.015) | (0.014) | (0.014) | (0.014) |
| Year fixed effects | ✓ | ✓ | ✓ | ✓ |
| Time since crime fixed effects | ✓ | ✓ | ✓ | ✓ |
| Firm fixed effects | ✓ | ✓ | ✓ | ✓ |
| Observations | 29,813 | 30,056 | 27,618 | 28,046 |
| Non-Violent Mean | 0.824 | 0.845 | 0.819 | 0.828 |

*Notes*: Table reports the impact of a violent incident between colleagues interacted with whether the firm is female-managed on victim and perpetrator employment outcomes separately for male-female and male-male crimes as specified by column headings. We define management as "male" if the share of men in the top 20% of earners is above the median share, and "female" otherwise. The reported DiD estimates use the matched control for the victim (or perpetrator) to identify effects 5 years after versus 5 years before a violent incident using the event study design, estimating equation 1, but adding the interaction term with female managers. Employment is measured at the end of the year. Sample construction and data as defined in Sections 2 and 3.

Our results suggest that female managers are less accommodating of perpetrators of workplace violence *in general*, and not just for male-female violence. The major difference is that male managers are simply much less likely to fire perpetrators in male-female violence compared with male-male violence. However, while female managers appear to be more likely to fire perpetrators after workplace violence, these actions alone do not mitigate the significant negative impacts on the direct victims.

In Table 6, we jointly examine the impact of perpetrators losing their job and of female management on the share of women employed in the firm. To capture perpetrator job loss, we introduce a dummy variable equal to 1 if the perpetrator loses their job in the post-violence period. Column (2) demonstrates that following male-female violence, perpetrators losing their job reduces the impact of colleague violence on the share of women employed at the firm (p-value=0.068), although this does not fully negate the full impact of the violence. It is interesting that it is only the interaction of female management and a perpetrator losing their job that mediates the impact of male-female violence on the wider workforce (p-value=0.069). Female-managed firms where the perpetrator remains employed do not have significantly different outcomes to male-managed firms, and male-managed firms where perpetrators leave still face significant falls in the proportion of women employed.

Overall, these results indicate different management practices by gender in resolving conflicts within the firm. One possible interpretation is that female managers have less tolerance for misbehavior, regardless of the gender of the misbehaving party. Alternatively, these differences could be consistent with an "in-group" tolerance. In other words, male managers may be more forgiving of male perpetrators. This latter explanation would be consistent with the findings from Egan *et al.* (2022) and Cullen and Perez-Truglia (2019). We are unable to disentangle these two explanations, and possibly both could be at work. However, given that men are overwhelmingly the perpetrators of workplace violence and are more likely to be managers in general (Bertrand and Hallock, 2001), "in-group" bias would likely tend to favor male perpetrators.

**Table 6:** Gender Composition of Management Impact on Share Female Employees

| | Dependant Variable: Share Female Employees in Firm | | | | | |
|---|---|---|---|---|---|---|
| | Male-Female | | | Male-Male | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treat*Female Manager | 0.0210 | | 0.0010 | 0.0047 | | -0.0186 |
| | (0.0082) | | (0.0129) | (0.0055) | | (0.0077) |
| Treat*Perpetrator JobLoss | | 0.0160 | 0.0046 | | 0.0048 | -0.0083 |
| | | (0.0087) | (0.0139) | | (0.0057) | (0.0079) |
| Treat*Female Manager*Perp JobLoss | | | 0.0288 | | | 0.0324 |
| | | | (0.0158) | | | (0.0101) |
| Treatment | -0.0196 | -0.0337 | -0.0214 | -0.0107 | -0.0111 | -0.0041 |
| | (0.0076) | (0.0083) | (0.0122) | (0.0046) | (0.0053) | (0.0075) |
| Year fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Time since crime fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Firm fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 17,964 | 17,964 | 17,964 | 16,999 | 16,999 | 16,999 |
| Dependant Variable Mean | 0.4042 | 0.4042 | 0.4042 | 0.2083 | 0.2083 | 0.2083 |

*Notes*: Table reports the impact of a violent incident between colleagues on the share of women employed in the firm. We define management as "male" if the share of men in the top 20% of earners is above the median share, and "female" otherwise. Perpetrator job loss is a dummy variable equal to one if the perpetrator becomes unemployed in the five years following the incident. The reported DiD estimates use the matched control for the firm to identify effects 5 years after versus 5 years before a violent incident using the event study design, estimating equation 1, but adding the interaction term with female managers. Sample construction and data as defined in Sections 2 and 3.

# 7 Discussion

Our results have a number of implications. First, female victims of workplace violence have few economic incentives to report violence at work. Even in the relatively severe cases reported to the police in our data, the male perpetrator experiences relatively small labor market costs for his actions. This is consistent with the vast under-reporting of workplace harassment and abuse suggested by survey data.[30] A major, known problem in preventing harassment at work is that

[30]"Based on anonymous survey responses, no fewer than 1 in 28 U.S. workers report having been victimized by workplace sexual harassment annually. Yet only 1 in 11,000 workers file a formal sexual harassment charge with the Equal Employment Opportunity Commission (EEOC), the agency tasked with enforcing all federal anti-discrimination laws." (Dahl and Knepper, 2021, p.1)

victims rarely report the problem to their employer (Magley, 2002). Women under-reporting harassment and violence at the hands of a colleague (and in particular one's manager) is easily reconciled with the comparative lack of career consequences for perpetrators of male-female violence we have documented.

Second, given that under-reporting is common, we are likely only observing a small fraction of all cases of workplace violence. As described in Section 2, just 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; European Institute for Crime Prevention & Control, 2009). Conservatively, this implies that the incidence of workplace violence is at least 10 times larger than can be documented by police reports. At the same time, under-reporting and selective reporting is relevant for the external validity of our results. While we provide the first evidence of the causal impacts of workplace violence on perpetrators, victims, and the broader firm, we can only do so for the (likely) more severe cases reported to police. We might not expect to see quite as large of impacts on victims, perpetrators, and the firm from less severe abuse by colleagues.

However, our effects sizes are extremely large. To put them in context, in Finland an exogenous job loss reduces employment over the next six years by 10.9 percentage points (Kaila *et al.*, 2022). This is only slightly larger than the employment effect for women of being victimized at work of 8.4 percentage points. Thus, even if less severe forms of harassment result in only a small fraction of the costs to victims, given that survey evidence suggests somewhere between one-tenth to half of all women experience harassment at work (Folke and Rickne, 2022) the overall impacts on female employment, female earnings, and the economy as a whole could be very large.

Third, our results suggest that relying on whisper networks and informal means to solve this problem is unlikely to work. This is true for two reasons. First, women face limited incentives to report, as described above. Thus, these crimes are likely to be largely invisible, particularly in cases where there were no witnesses. Second, we find no reduction in within network hiring. This is consistent with information about these events failing to circulate beyond the victim and perpetrator, preventing informal networks like "whisper networks" from solving these issues.

Fourth, the firm responses we have documented have potentially broader implications for sorting across firms. The fact that male-female violence leads male-managed firms to change their workforce composition towards male employees could partially segment the workforce, leading to male-dominated workplaces where male-management repeatedly allows perpetrators of male-female violence to remain employed at the expense of female employees. This would likely entail an equilibrium in which women in the firm's gender minority are also more likely to find themselves in firms that tolerate abuse and harassment of women. Such a result is consistent with the descriptive facts documented in Folke and Rickne (2022). More generally, this type of sorting relates to research in economics that increasingly recognizes the work environment as an important source of labor market inequality and gender heterogeneity (Le Barbanchon *et al.*, 2021; Mas and Pallais, 2017).

Last, given the large impacts on victims, a natural question is whether we can predict who commits these crimes. Prediction combined with preventative actions could allow firms to avoid such incidents. In Appendix A, we explore whether recent machine learning techniques can predict perpetrators of workplace violence. Using a causal forest supervised machine learning algorithm, we find that this method does a decent job of predicting perpetrators. On average those who perpetrate events have a predicted probability of perpetrating violence of 0.2230 compared with a probability of 0.0143 among those who do not commit violence. We also calculated the AUC (Area Under the ROC Curve), which is more than 85%. We provide more details in Appendix A. This exercise is intended to illustrate that if firms are serious about eliminating violence against women at work, it might be possible to at least make some progress, although testing effectiveness of different measures is beyond the scope of this paper.[31] However, the unwillingness to fire perpetrators of such events after they happen amongst male-managed firms may reflect a more general unwillingness to act on the part of these firms, rendering this exercise irrelevant.

[31]There exists promising research suggesting that interventions could be effective. For example Antecol and Cobb-Clark (2003) find that training increases understanding about what constitutes harassment at work.

## 8 Conclusion

In this paper we estimated the impacts of workplace violence on victims, perpetrators, and the broader firm. We find that workplace violence has large negative impacts on victims and perpetrators. However, male perpetrators of male-female violence experience less severe repercussions compared with perpetrators of male-male violence. This is partly, but not fully, explained by the fact that male-female violence is more likely to be characterized by greater economic inequality between victims and perpetrators.

We motivated this paper in part by asking whether the anecdotal and high-profile #MeToo cases of male-female workplace violence, characterized by female victims experiencing larger costs than their male perpetrators, were the exception or the rule. Did #MeToo perpetrators tend to get away with their crimes because they were famous and powerful? Or rather, do more obscure individuals enjoy similar immunity? Our results show that relative power within the firm, even in the everyday cases, plays a key role in insulating male perpetrators of violence against women at work.

We also show that male-female violence has broader implications for women in the firm in general and not just for the female victim. Following male-female violence, firms become significantly more male, with no such repercussions following male-male violence. This is explained both by a reduction in the share of female new hires as well as women leaving the firm. We find that the drop in new female hires is unlikely to be a supply side phenomenon as there is no impact on hiring within employee networks. This result suggests that informal "whisper networks" are unlikely to resolve these issues.

However, the results from this paper do provide one optimistic takeaway: the composition of management can reduce the broader impacts on the firm. Specifically, we find that female managers are able to mitigate the impacts of male-female workplace violence on other female employees within the firm. They accomplish this in part by being more likely to fire the perpetrators of these crimes. Thus, there is a way to reduce the costs of violence against women at work, namely by ensuring that violent actions against colleagues result in consequences for the perpetrators. While this may seem an obvious response, our results demonstrate that this is not done consistently, particularly in male-managed firms after male-female crimes. Such lack of consequences not only benefits perpetrators at the cost of their victims, but are also costly to women in general, as they are less likely to be employed within these relatively high-paying firms in the future.

Our results suggest several avenues for new research. First, data constraints make it impossible for us to explore the impacts of lower-level bullying and harassment as they are not reported to police. However, obtaining such data and understanding if lower-level harassment has similar impacts on perpetrators, victims, and the broader firm would be informative. Second, our results add to a growing literature on management practices. Understanding differences in how male versus female decision-makers manage firms beyond just conflict between colleagues could reveal other important implications for the workforce. Last, our analysis is suggestive that there is a "business case" for preventing violence and harassment against women, beyond the obvious ethical one, although we do not quantify this cost. We show that turnover rates of women and hiring are significantly effected by male-female violence in male managed firms. In the face of turnover and hiring costs, these changes could potentially be very costly, beyond the impact on the loss of talent.

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# Online Appendix

## A Predicting Perpetrators with Machine Learning

A relevant policy question given the large impacts of between colleague violence on victims and firms is how can such incidents be prevented? One way to prevent these incidents is to first predict who is most at risk of committing violence against their colleagues, and then intervene in some way to reduce the risk. While evaluating the effectiveness of interventions is beyond the scope of this paper, in this section we provide suggestive evidence on how accurately one can predict the workers who are most likely to perpetrate violence.

For this exercise, we use a causal forest supervised machine learning (ML) algorithm, specifically implementing the generalized random forest approach from Athey *et al.* (2019) to predict outcomes. This methodology has the benefit of comparing multiple possible models, with built in "tuning" to select the best predictive model based on the data. While these results cannot be interpreted causally, we view this exercise as potentially providing a reasonable tool to help identify workers at risk of violence. This is a necessary first step to preventing these violent events.

When we apply our ML estimates to the holdout sample we find that on average those who perpetrate events have a predicted probability of perpetrating violence of 0.2230 compared with a probability of 0.0143 among those who do not commit violence. The AUC (Area Under the ROC Curve) is more than 85%. In Table B.8 we turn to the variable importance to understand which variables play the most important role in predicting violence. This table calculates a value based on the percent of trees in the causal forest that use the given characteristic. From the table, we find by far the most important characteristic to predict future violence is the number of past crimes. There is a charged debate about whether criminal history should be included in the information employers are allowed to observe (Doleac and Hansen, 2020; Craigie, 2020; Agan and Starr, 2018). However, given this is part of the information available to employers in at least some locations, it is useful to see how it might be used in this context. After a criminal record, the other important variables that are highly predictive of an employee committing violence against a colleague are gender, education level, and the share of women in the firm.

How could a firm use this information? Theoretically, a firm could potentially use the predicted probability of violence that emerges for each individual from the ML algorithm to identify a cutoff predicted value, and for individuals above the cutoff implement some sort of intervention, such as a monthly check in discussion with HR. Whether or not such a step would be useful first depends on a) does the ML cutoff identify a large percentage of those who could go on to commit violence and b) does the cutoff reduce the incidence of false positives, defined as having a low probability of including individuals who will not commit violence. Inherently, the ideal cutoff makes a trade-off between these two features.

Figure C.17 depicts the predicted probabilities for the hold-out sample on the x-axis. It then graphs the percent of those who commit violence against a colleague that we actually detect above a given threshold (the "True Positive Rate") as well as the percent of those who do not go on to commit violence, but who would be identified as at risk by the the predicted probability threshold (the "False Positive Rate"). Note that among the latter group it is possible that some are still causing incidences that do not rise to the level of a police report. As an example of how the algorithm performs, if we use a ML predicted probability threshold cut-off of 0.2, the algorithm identifies almost half of those who go on to commit violence against colleagues (where in this case we know in the holdout sample whether individuals actually commit violence that leads to a police report or not), and has close to a zero false positive rate. This suggest that with reasonable thresholds, the algorithm does a good job of identifying those who will go on to commit violence without falsely identifying large portions of those who never commit violence. Thus, this could be a useful tool for firms to reduce future violence amongst their employees if paired with effective programs to prevent violence.

# B Additional Tables

**Table B.1:** Sample Means for Colleague and Non-Colleague Assault

| | Male-Male | | | | Male-Female | | | |
|---|---|---|---|---|---|---|---|---|
| | Colleague | | Non-Colleague | | Colleague | | Non-Colleague | |
| | Perp | Victim | Perp | Victim | Perp | Victim | Perp | Victim |
| Age | 34.75 | 33.94 | 30.35 | 30.94 | 40.98 | 37.64 | 38.63 | 35.99 |
| Share college | 0.05 | 0.06 | 0.03 | 0.04 | 0.08 | 0.12 | 0.06 | 0.07 |
| Share high school | 0.61 | 0.58 | 0.44 | 0.43 | 0.59 | 0.57 | 0.49 | 0.47 |
| Share dropouts | 0.34 | 0.36 | 0.53 | 0.53 | 0.32 | 0.31 | 0.44 | 0.46 |
| Employment | 0.96 | 0.88 | 0.44 | 0.41 | 0.95 | 0.87 | 0.48 | 0.42 |
| Earnings | 32,300.75 | 30,830.25 | 12,029.28 | 12,570.37 | 30,648.07 | 21,793.15 | 15,154.68 | 10,898.13 |
| Positive earnings | 33,620.56 | 32,430.91 | 18,821.99 | 19,513.73 | 35,490.46 | 25,373.14 | 24,557.60 | 18,316.89 |

*Notes*: Table reports sample means for all perpetrators and victims of colleague and non-colleague assaults separately for male-female and male-female crimes. Data is from the police reports linked to FLEED register data.

**Table B.2:** Descriptive Regressions

| | All | Male-Female | Male-Male |
|---|---|---|---|
| Age Quartile=2 | 0.00036 | 0.00016 | 0.00032 |
| | (0.00036) | (0.00025) | (0.00024) |
| Age Quartile=3 | -0.00025 | -0.00017 | 0.00003 |
| | (0.00037) | (0.00025) | (0.00025) |
| Age Quartile=4 | -0.00224*** | -0.00133*** | -0.00078*** |
| | (0.00042) | (0.00029) | (0.00028) |
| Prop. College | -0.00611*** | -0.00239*** | -0.00364*** |
| | (0.00058) | (0.00040) | (0.00039) |
| Income Quartile=2 | -0.00243*** | -0.00183*** | -0.00017 |
| | (0.00057) | (0.00039) | (0.00038) |
| Income Quartile=3 | -0.00292*** | -0.00222*** | -0.00040 |
| | (0.00057) | (0.00039) | (0.00039) |
| Income Quartile=4 | -0.00160*** | -0.00173*** | 0.00038 |
| | (0.00060) | (0.00041) | (0.00040) |
| Size Quartile=2 | -0.00067 | -0.00012 | -0.00051 |
| | (0.00070) | (0.00048) | (0.00047) |
| Size Quartile=3 | -0.00028 | -0.00018 | -0.00019 |
| | (0.00063) | (0.00043) | (0.00042) |
| Size Quartile=4 | 0.00521*** | 0.00217*** | 0.00266*** |
| | (0.00061) | (0.00042) | (0.00041) |
| Turnover | -0.00030 | -0.00004 | -0.00046 |
| | (0.00060) | (0.00041) | (0.00040) |
| Share Female | -0.00230*** | 0.00133*** | -0.00416*** |
| | (0.00060) | (0.00041) | (0.00040) |
| Gender Pay Gap (rel. Av. Income) | 0.00009 | 0.00021 | -0.00018 |
| | (0.00026) | (0.00018) | (0.00017) |
| Share Female Managers | -0.00046 | -0.00059** | -0.00016 |
| | (0.00036) | (0.00025) | (0.00024) |
| Constant | 0.00487*** | 0.00215** | 0.00236*** |
| | (0.00132) | (0.00091) | (0.00089) |
| Observations | 366,664 | 366,664 | 366,664 |
| $R^2$ | 0.0042 | 0.0018 | 0.0035 |

*Notes*: Table reports descriptive LPM regressions where the outcome is a dummy that is 1 if there is any workplace violence incident in the firm in the first column, and male-female (male-male) in the second (third) columns. Proportion college indicates proportion with a masters degree (equivalent to college in Finland). Age quartiles are dummies for the categories with the bottom quartile as the omitted category. Income quartiles divide firms into four equally sized groups by the average income paid to employees. Similarly for firm size. The gender pay gap is the gap in the average earnings of men and women in the firm, divided by the average pay of women.

**Table B.3:** Employment Effects for Victims and Perpetrators & Robustness Checks

| | Dependant Variable: Employment | |
|---|---|---|
| | Victim | Perpetrator |
| | (1) | (2) |
| **Panel A: Male-Female** | | |
| Main estimates | -0.084 | -0.052 |
| | (0.012) | (0.011) |
| Robustness to overfitting | -0.084 | -0.047 |
| | (0.011) | (0.011) |
| Placebo check | 0.003 | 0.003 |
| | (0.013) | (0.009) |
| Employment mean | 0.824 | 0.845 |
| Observations | 29,813 | 30,056 |
| **Panel B: Male-Male** | | |
| Main Estimates | -0.042 | -0.106 |
| | (0.012) | (0.012) |
| Robustness to overfitting | -0.050 | -0.090 |
| | (0.011) | (0.012) |
| Placebo check | -0.012 | 0.002 |
| | (0.011) | (0.010) |
| Employment mean | 0.819 | 0.828 |
| Observations | 27,618 | 28,046 |
| Year fixed effects | ✓ | ✓ |
| Time since crime fixed effects | ✓ | ✓ |
| Individual fixed effects | ✓ | ✓ |
| Age by time since crime | ✓ | ✓ |

*Notes*: Table reports estimates of $\delta_t$ obtained using Equation (1) where we collapse into a pre- and post-period to recover difference-in-difference estimates. Column (1) estimates the impacts for victims while column (2) reports estimates for perpetrators. Employment is measured at the end of the year. Data is from police reports linked to FLEED administrative register data. See Section 2 for more details on sample construction.

**Table B.4:** Employment and Income Effects for Victims and Perpetrators

| Dependant Variable: | Employment<br>(1) | Income<br>(2) | % Income<br>(3) |
|---|---|---|---|
| **Panel A: Male-Female Crimes** | | | |
| Victims DiD Estimates | -0.084 | -2,198.304 | -0.159 |
| | (0.012) | (487.514) | (0.047) |
| Overfitting Estimate | -0.084 | -2,414.387 | -0.132 |
| | (0.011) | (515.030) | (0.046) |
| Perpetrators DiD Estimates | -0.052 | -4,189.828 | -0.081 |
| | (0.011) | (880.159) | (0.043) |
| Overfitting Estimate | -0.047 | -3,182.633 | -0.074 |
| | (0.011) | (863.154) | (0.043) |
| Victim Mean | 0.824 | 23,420.201 | 1.410 |
| Perpetrator Mean | 0.845 | 33,195.316 | 1.331 |
| Observations | 29,813 | 29,813 | 29,813 |
| **Panel B: Male-Male Crimes** | | | |
| Victims DiD Estimates | -0.042 | -1,413.326 | -0.056 |
| | (0.012) | (666.797) | (0.043) |
| Overfitting Estimate | -0.050 | -1,293.540 | -0.019 |
| | (0.011) | (670.981) | (0.041) |
| Perpetrators DiD Estimates | -0.106 | -3,651.404 | -0.213 |
| | (0.012) | (619.153) | (0.040) |
| Overfitting Estimate | -0.090 | -2,402.600 | -0.178 |
| | (0.012) | (658.237) | (0.041) |
| Victim Mean | 0.819 | 32,302.481 | 1.196 |
| Perpetrator Mean | 0.828 | 33,007.172 | 1.301 |
| Observations | 27,618 | 27,618 | 27,618 |
| Year fixed effects | ✓ | ✓ | ✓ |
| Time since crime fixed effects | ✓ | ✓ | ✓ |
| Individual fixed effects | ✓ | ✓ | ✓ |
| Age by time since the event | ✓ | ✓ | ✓ |

*Notes*: Table reports estimates of $\delta_t$ obtained using Equation (1) where we collapse into a pre- and post-period to recover difference-in-difference estimates. Panel A estimates impacts for male-female violence while Panel B reports estimates for male-male violence. Employment measured at end of the year. Income corresponds to total taxable income at year end. % income measures yearly income as a fraction of the yearly income 1 year prior to the incident. See Section 2 for more details on sample construction.

**Table B.5:** The Effect of Workplace Violence on Employment for Male-Female Violence: Heterogeneity by Individual Characteristics

| | Dependant Variable: Employment | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Victim**: | | | | |
| Treatment*Age | 0.000 | | | |
| | (0.001) | | | |
| Treatment*Income | | 0.014 | | |
| | | (0.005) | | |
| Treatment*Manager | | | -0.015 | |
| | | | (0.033) | |
| Treatment*Tenure | | | | 0.001 |
| | | | | (0.004) |
| Treatment | -0.085 | -0.213 | -0.083 | -0.093 |
| | (0.043) | (0.053) | (0.012) | (0.043) |
| N. of Obs. | 29,813 | 29,813 | 29,813 | 29,813 |
| **Perpetrators** | | | | |
| Treatment*Age | 0.003 | | | |
| | (0.001) | | | |
| Treatment*Income | | 0.007 | | |
| | | (0.009) | | |
| Treatment*Manager | | | 0.024 | |
| | | | (0.043) | |
| Treatment*Tenure | | | | 0.008 |
| | | | | (0.004) |
| Treatment | -0.170 | -0.125 | -0.053 | -0.141 |
| | (0.044) | (0.095) | (0.011) | (0.042) |
| N. of Obs. | 30,056 | 30,056 | 30,056 | 30,056 |
| Year fixed effects | ✓ | ✓ | ✓ | ✓ |
| Time since crime fixed effects | ✓ | ✓ | ✓ | ✓ |
| Individual fixed effects | ✓ | ✓ | ✓ | ✓ |
| Age by time since the event | ✓ | ✓ | ✓ | ✓ |

*Notes*: Table reports estimates of $\delta_t$ obtained using Equation (2) where we collapse into a pre- and post-period to recover difference-in-difference estimates. Treatment is interacted wuth individual characteristics. Employment is measured at the end of the year. Data is from police reports linked to FLEED administrative register data. See Section 2 for more details on sample construction.

**Table B.6:** Impacts of Assaults

| Dependant Variable: | Victim Employment (1) | Perpetrator Employment (2) |
|---|---|---|
| **Panel A: Male-Female** | | |
| Treatment*Assault | 0.001 | -0.004 |
| | (0.017) | (0.016) |
| Treatment | -0.085 | -0.050 |
| | (0.015) | (0.014) |
| Observations | 29,813 | 30,056 |
| Dependant Variable Means | 0.823 | 0.845 |
| **Panel B: Male-Male** | | |
| Treatment*Assault | -0.014 | -0.022 |
| | (0.018) | (0.019) |
| Treatment | -0.034 | -0.092 |
| | (0.016) | (0.016) |
| Observations | 27,618 | 28,046 |
| Dependant Variable Means | 0.819 | 0.828 |
| Year fixed effects | ✓ | ✓ |
| Time since crime fixed effects | ✓ | ✓ |
| Individual fixed effects | ✓ | ✓ |
| Age x time since crime | ✓ | ✓ |

*Notes*: Table reports difference-in-difference estimates from Equation (2) where we collapse into a pre- and post-period to recover DiD estimates. In all cases the dependent variable is employment measured at year end. Panel A reports estimates for only male-female workplace violence while Panel B report estimates for male-male workplace violence. The only interaction term included in this table is for whether the crime is an assault, which is denoted by the crime category in the police data, and where we take the most serious crime to define the crime type in cases where multiple crime types are reported in a single report.

**Table B.7:** Impact of Workplace Violence on Firm Outcomes

| | Male-Female | | Male-Male | |
|---|---|---|---|---|
| Dependant Variable: | Firm Size<br>(1) | Plant Closing<br>(2) | Firm Size<br>(3) | Plant Closing<br>(4) |
| DiD Estimate | -52.185<br>(49.720) | 0.011<br>(0.013) | -6.622<br>(15.646) | -0.005<br>(0.011) |
| Year fixed effects | ✓ | ✓ | ✓ | ✓ |
| Time since crime fixed effects | ✓ | ✓ | ✓ | ✓ |
| Firm fixed effects | ✓ | ✓ | ✓ | ✓ |
| Observations | 17,964 | 19,668 | 16,999 | 17,974 |
| Non-Violent Mean | 214.129 | 0.084 | 168 | 0.055 |

*Notes*: Table reports the impact of a violent incident between colleagues on the firm size, i.e. the total number of employees in the firm in columns (1) and (3) and firm exit, which is equal to 1 if the firm does not appear in the data, in columns (2) and (4). The table reports DiD estimates using the matched control firm to identify effects 5 years after versus 5 years before a violent incident against a colleague using equation (1) collapsed into a pre- and post-period. Firm size and exit are measured at the end of the year. Sample construction and data as defined in Sections 2 and 3.

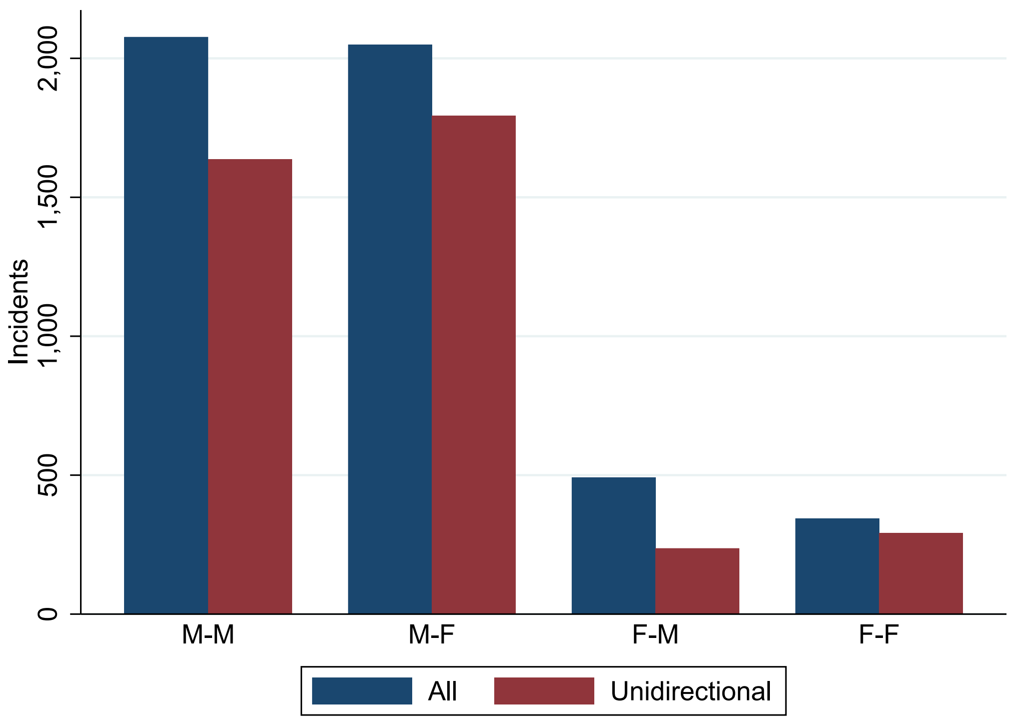
**Table B.8:** Machine Learning Variable Importance

| Variable | Importance |
|---|---|
| **Individual features** | |
| Number Past Crimes | 0.69211 |
| Gender | 0.12872 |
| Education Level | 0.09191 |
| Age | 0.0069 |
| All Income | 0.00231 |
| Capital Income | 0.0012 |
| Number Children | 0.00017 |
| Entrepreneurial Earnings | 0.00013 |
| Helsinki | 6e-05 |
| **Firm features** | |
| Firm Share Female | 0.04448 |
| Firm Mean Earnings | 0.02039 |
| Firm Mean Age | 0.01065 |
| Firm Employee Mean Tenure | 0.00097 |

*Notes*: The table shows the importance of each variable in the Machine Learning algorithm. See Appendix A and Athey *et al.* (2019) for more details.

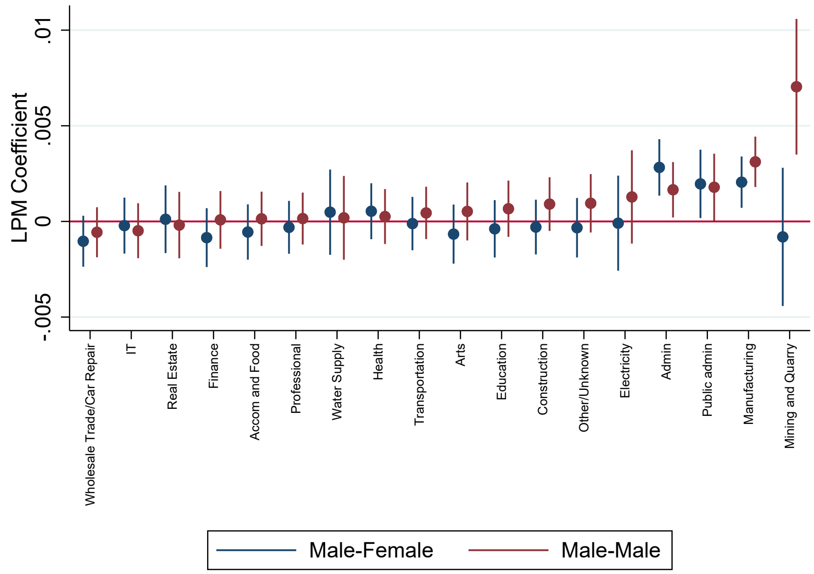
## C Additional Figures

**Figure C.1:** Gender Breakdown of Workplace of Cases of Workplace Violence




*Notes:* Figure shows the number of cases within each violence category in blue bars for: male-male, male-female, female-male, and female-female violence where, for example, male-female violence cases consist of a male perpetrator and a female victim. In red bars we indicate the number of cases where the perpetrator is not also listed as a victim in the police data. For example, in a bar fight between two equally culpable parties, both parties could be listed as victims and perpetrators.

**Figure C.2:** Industries Where Between Colleague Violence Occurs

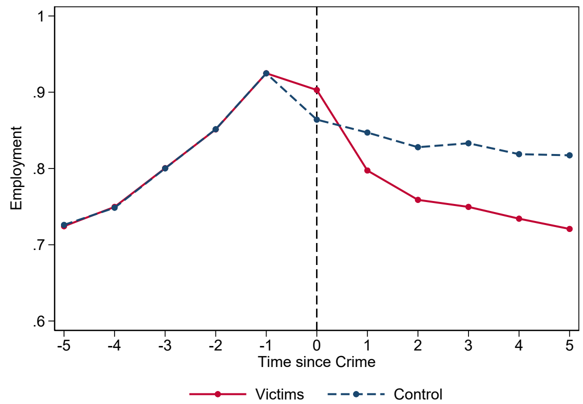



*Notes:* Figure reports estimates of LPM regressions where we regress industry dummies on dummies indicating whether male-female (in blue) and male-male (in red) violence between colleagues took place.

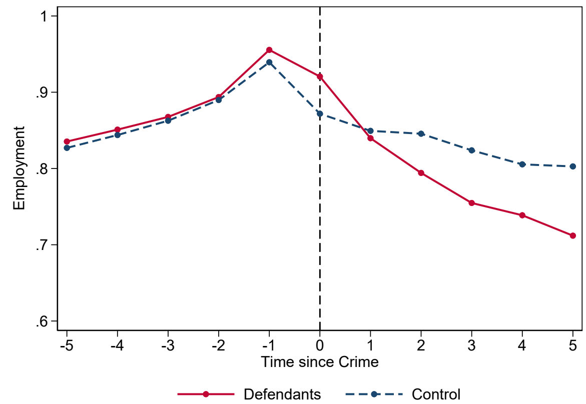
**Figure C.3:** Raw Patterns of Employment for Victims and Perpetrators (and Their Matches) Before and After Colleague Violence

Panel I: Male-Female Violence

(a) Victim

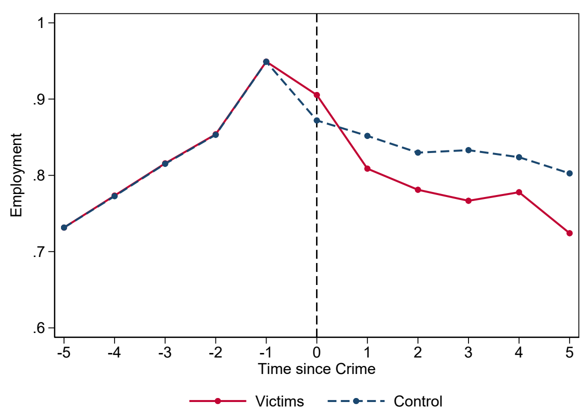



(b) Perpetrator

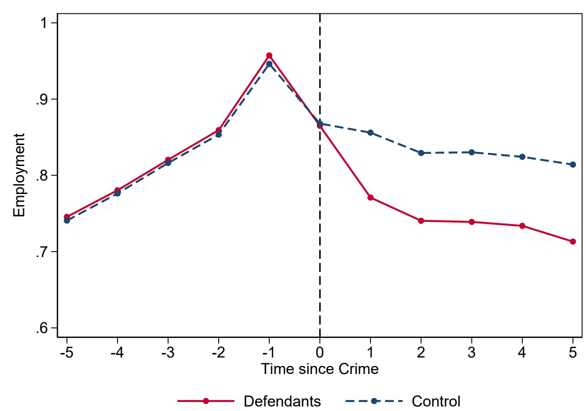



Panel II: Male-Male Violence

(c) Victim




(d) Perpetrator

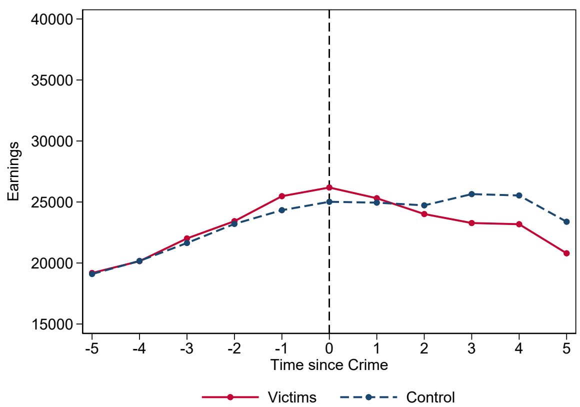



*Notes:* Panel I shows average employment for male-female violence for victims (left) and perpetrators (right). Panel II shows the same but for male-male crimes. Victim and perpetrator averages are depicted in the red lines and their matched controls are depicted in dashed blue lines. Raw averages reported 5 years before and 5 years after the violent incident. Employment and income are both measured at the end of the year, and income is measured in Euros. Sample construction and data as defined in Sections 2 and 3.

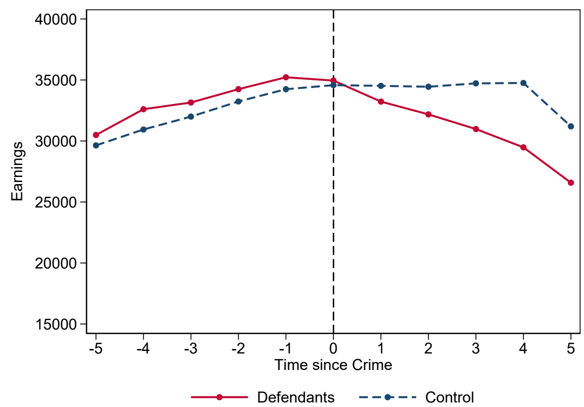
**Figure C.4:** Raw Patterns of Income for Victims and Perpetrators (and Their Matches) Before and After Colleague Violence

Panel I: Male-Female Violence

(a) Victim

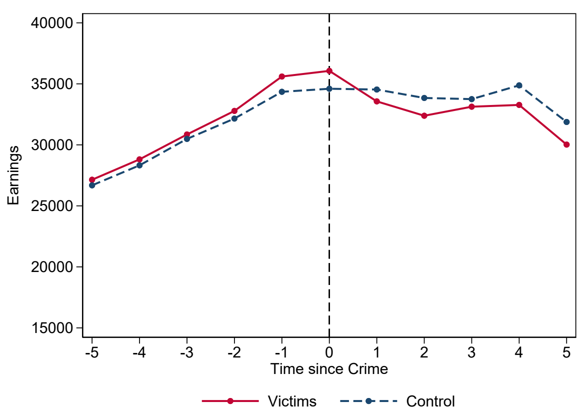



(b) Perpetrator

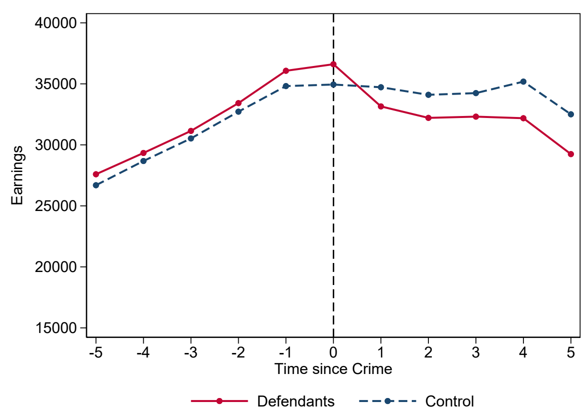



Panel II: Male-Male Violence

(c) Victim




(d) Perpetrator

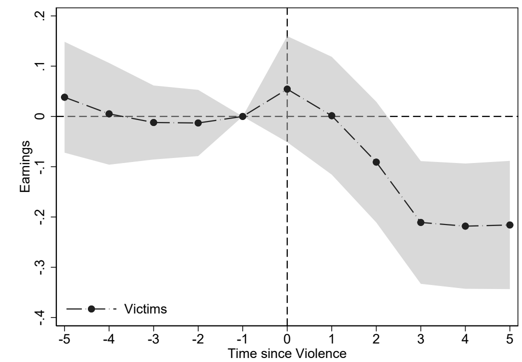



*Notes:* Panel I shows average income for male-female violence for victims (left) and perpetrators (right). Panel II shows the same but for male-male crimes. Victim and perpetrator averages are depicted in red lines and their matched controls are depicted in dashed blue lines. Raw averages reported 5 years before and 5 years after the violent incident. Income measured at the end of the year in Euros. Sample construction and data as defined in Sections 2 and 3.

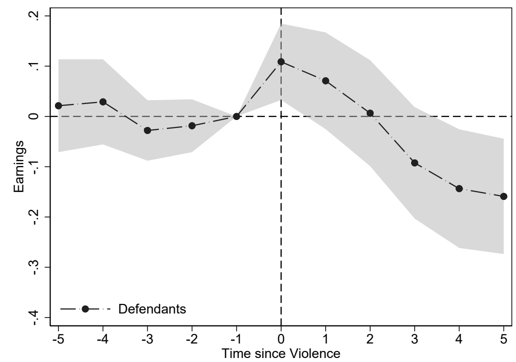
**Figure C.5:** Impact of Colleague Violence on Income of Victims and Perpetrators

Panel I: Male-Female Violence

(a) Victim

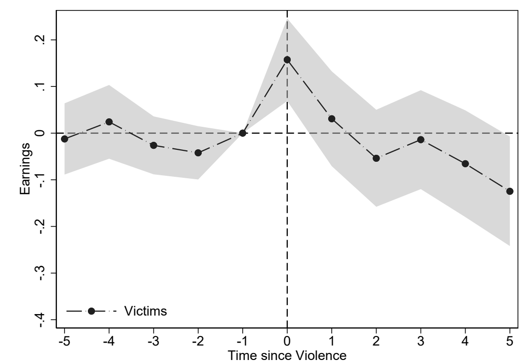



(b) Perpetrator

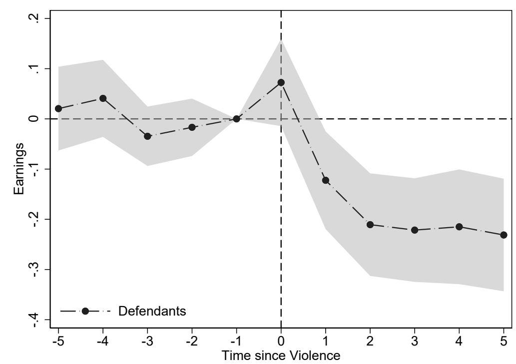



Panel II: Male-Male Violence

(c) Victim

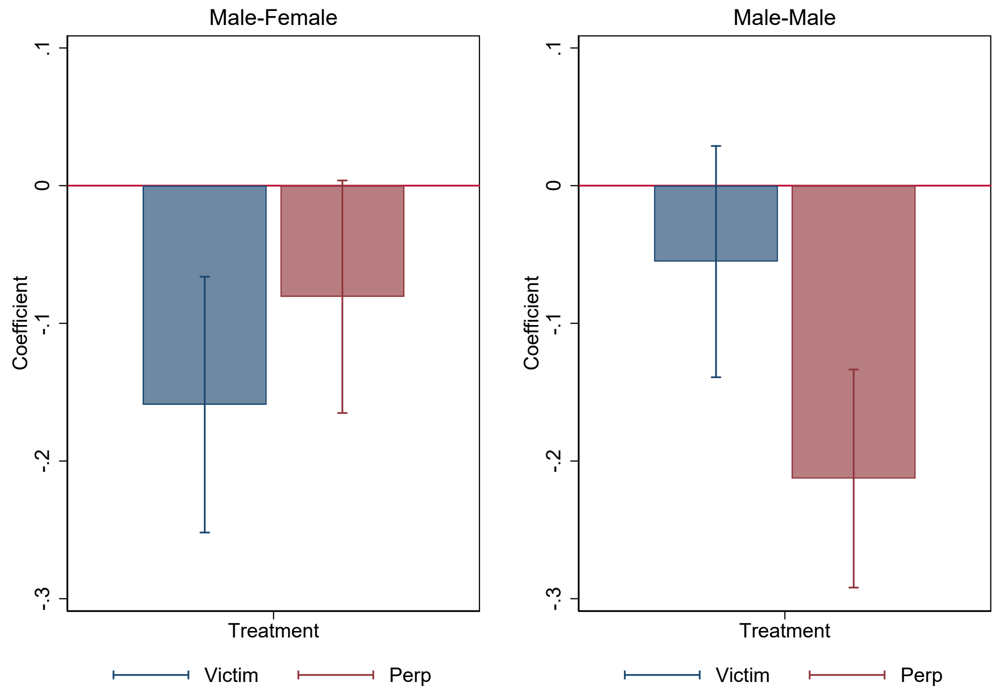



(d) Perpetrator



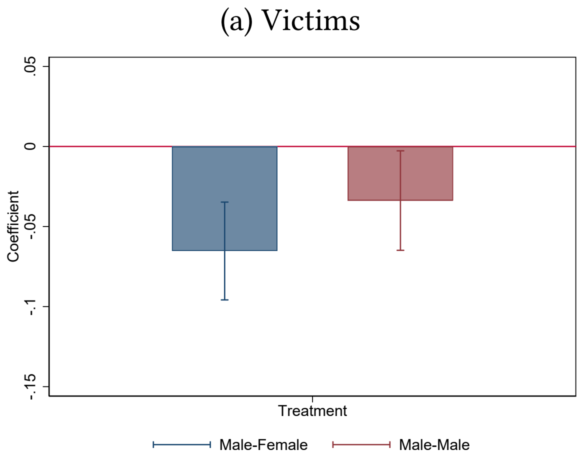

*Notes:* Each figure reports the impact of a violent incident between colleagues that results in a police report on income of the victim (left-hand side) or perpetrator (right-hand side). Income is measured as the income in year $t$ as a fraction of the income in the year before the event. First row reports results for male-female workplace crimes. Second row reports effects for male-male workplace crimes. The estimates use the matched control and the event study framework from equation 1 to identify effects 5 years before and 5 years after a violent incident against a colleague. Income each year includes total taxable income and is measured in December of each year. Sample construction and data as defined in Sections 2 and 3.

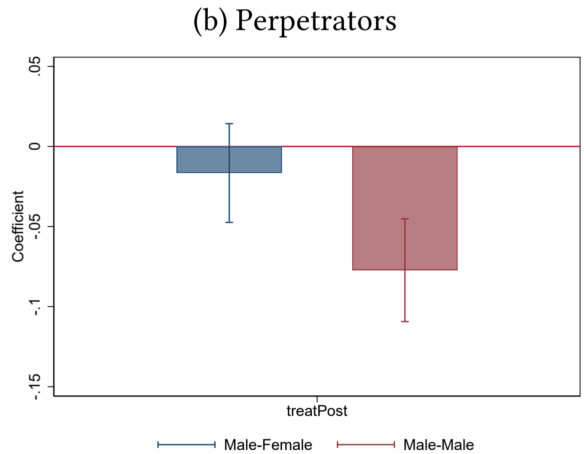
**Figure C.6:** Asymmetry in Impacts of Workplace Violence on Percent Income Changes




*Notes:* Figure reports estimates of $\delta_t$ obtained using Equation (1) where we collapse into a pre- and post-period to recover difference-in-difference estimates. Left-hand figure reports DiD estimates for male-female crimes for victims (in the blue bar on the left) and perpetrators (in the red bar on the right). Similarly for male-male crimes in the right-hand figure. 95% confidence intervals depicted in whiskers around the estimates. Outcome is percent of income, which measures all taxable income at the end of the year as a fraction of the total income in the year before the incident.

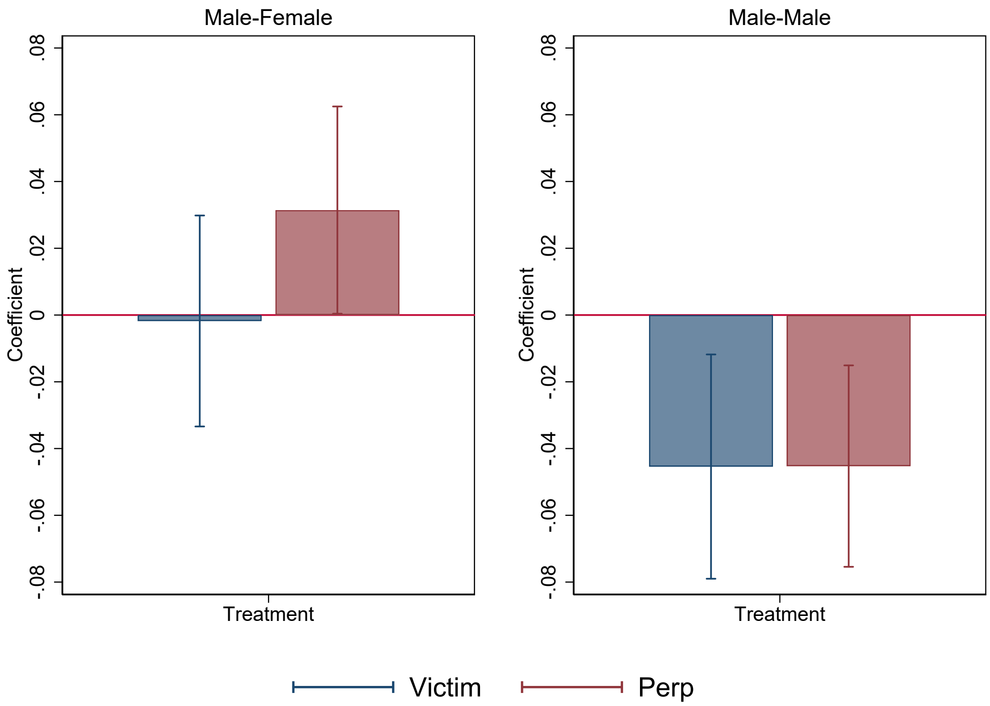
**Figure C.7:** Workplace Transitions



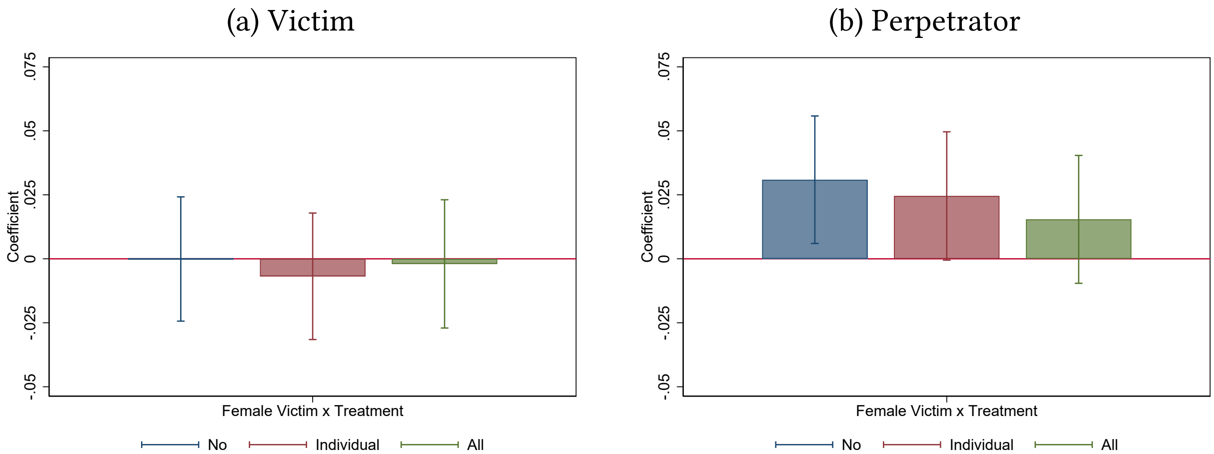

*Notes:* Figure (a) shows the DiD estimates with the dependent variable equal to whether the victim of workplace violence is in the same firm for male-female violence (left bar in blue) and for male-male violence (right bar in red). Figure (b) shows the DiD estimates with the dependent variable equal to whether the perpetrator of workplace violence is in the same firm for male-female violence (left bar in blue) and for male-male violence (right bar in red).

**Figure C.8:** Comparing Workplace and Non-Workplace Violence Impacts




*Notes:* Figure reports estimates of $\delta_t$ obtained using Equation (1) where we collapse into a pre- and post-period to recover difference-in-difference estimates. However, unlike the main estimates which uses a nearest neighbor match who is not also a victim (or perpetrator when estimating perpetrator impacts) of a crime, in this analysis we compare outcomes to a nearest neighbor match who was also a victim (or perpetrator) of one of the same main types of crimes in Table 1. Left-hand figure reports DiD estimates for male-female crimes for victims (in the blue bar on the left) and perpetrators (in the red bar on the right) compared with the impacts for non-workplace victims and perpetrators. Similarly for male-male crimes in the right-hand figure. 95% confidence intervals depicted in whiskers around the estimates. Employment is measured at the end of the year. See Sections 2 and 3.1 for more details.

**Figure C.9:** Controlling for Individual Characteristics and Power Imbalances, Impacts on Victims and Perpetrators of Male-Female Versus Male-Male Colleague Violence

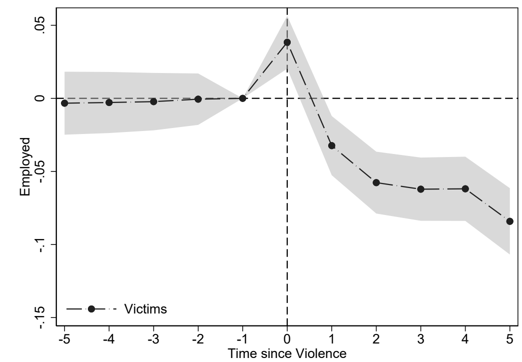



*Notes:* Figure (a) shows the impact of a violent incident against a colleague that results in a police report on the victim of male-female violence relative to male-male violence with no controls (blue bar on the left), with individual controls (red bar in the center) and with individual controls and relationship between victim and perpetrator controls (green bar on the right). Figure (b) shows the same but for perpetrators of male-female relative to male-male between colleague violence.

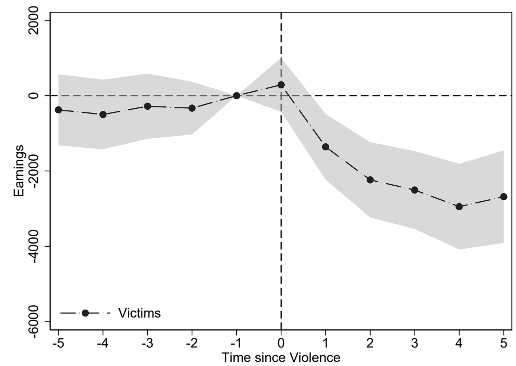
**Figure C.10:** Robustness of Victim Impacts to Overfitting and Future Victims Counterfactual

Panel I: Main Result, All Workplace Violence

(a) Employment

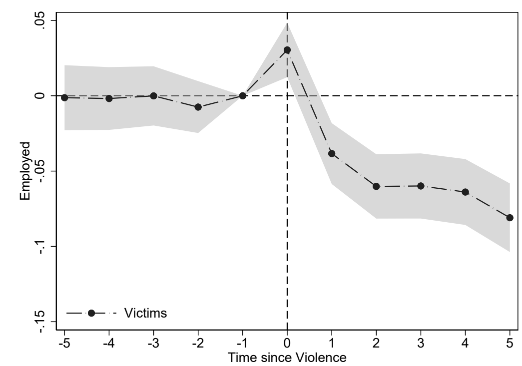



(b) Income

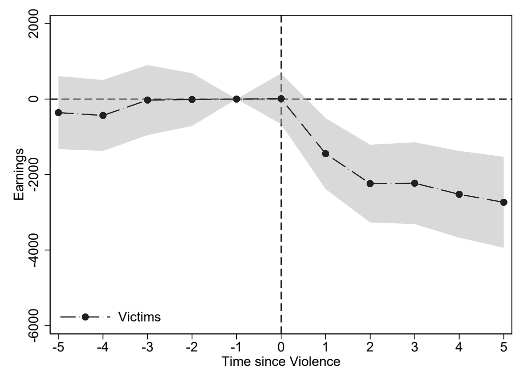



Panel II: Overfitting Adjustment

(c) Employment

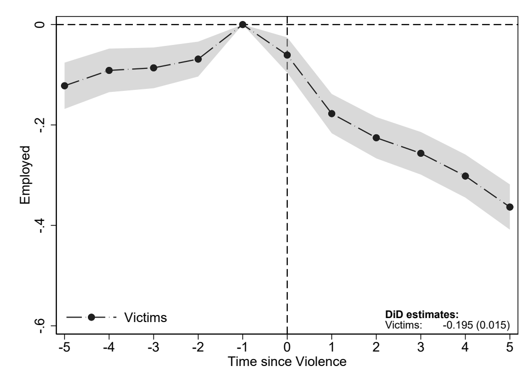



(d) Income

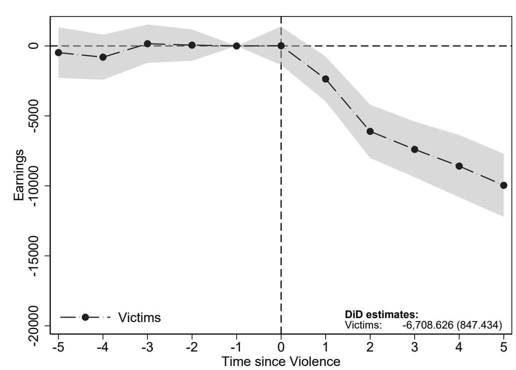



Panel III: Future Victims as Counterfactual

(e) Employment




(f) Income

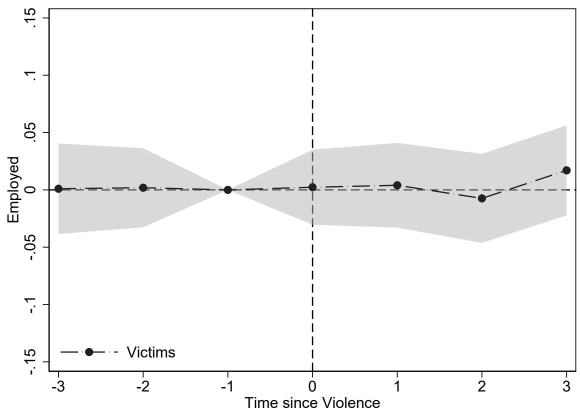



*Notes:* Each figure reports event study estimates of the impact of a violent incident between colleagues that results in a police report on victim employment (left-hand side) or income (right-hand side). Panel I repeats the main estimates for all workplace crimes. Panel II reports results where we address the possibility of overfitting by dropping two of the pre-period years when matching. Panel III reports estimates using future victims who are attacked by a colleague in a year beyond the post period in a stacked event study as the control to identify effects 5 years before and 5 years after a violent incident. Specifically, we take victims from 2014-2016 as the counterfactual for victims from 2006-2008, thus treatment effects do not overlap in the post period. Employment and income are both measured at the end of the year, and income is measured in Euros. See Section 4.3 for more details.

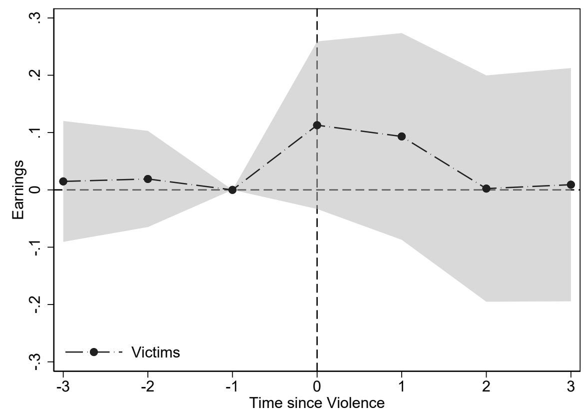
**Figure C.11:** Placebo Estimates of the Impact on Victim Matching 5 Years Prior to Event

Panel I: Male-Female Violence Placebo Victim Effects

(a) Employment

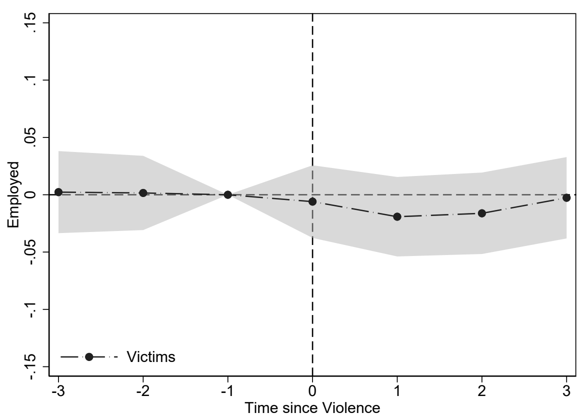



(b) Income

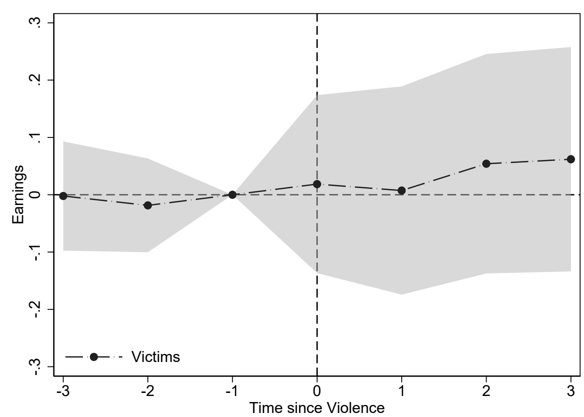



Panel II: Male-Male Violence Placebo Victim Effects

(c) Employment




(d) Income

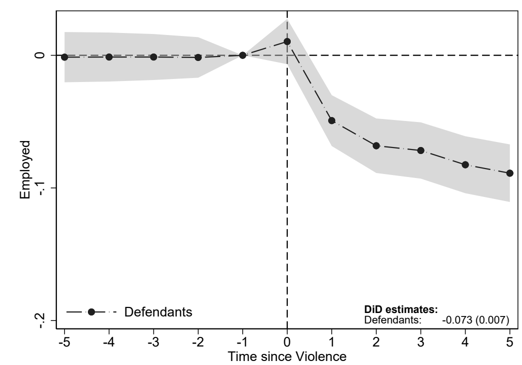



*Notes:* Panel I shows the impact of a placebo event 5 years prior to a male-female violent incident that results in a police report on the female future victim's employment relative to her matched control in subfigure (a) and the female future victim's income in subfigure (b). In Panel II we report the placebo results for male victims of male-male crimes. Employment and income are both measured at the end of the year, and income is measured in Euros. The placebo exercise moves the "event" line 5 years prior to the actual violent event, redoes the nearest neighbor matching to find a counterfactual, and re-estimates equation 1 to calculate effects 3 years before and after this placebo event. For more details, see Section 4.3.

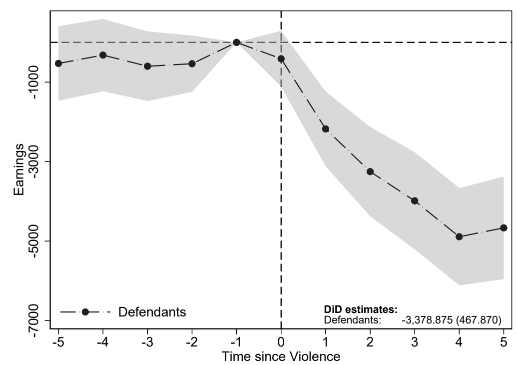
**Figure C.12:** Robustness of Perpetrator Impacts to Overfitting and Alternative Counterfactual

Panel I: Main Result, All Workplace Violence

(a) Employment

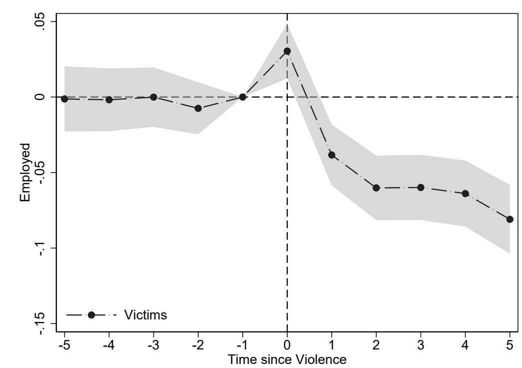



(b) Income

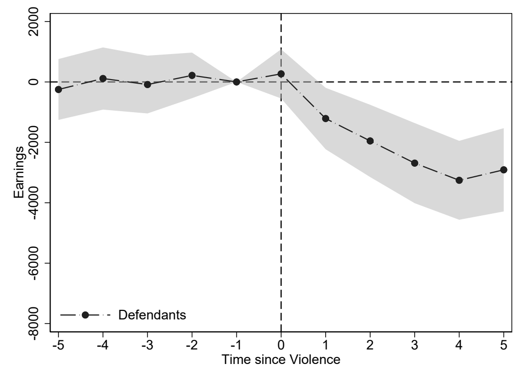



Panel II: Overfitting Adjustment

(c) Employment

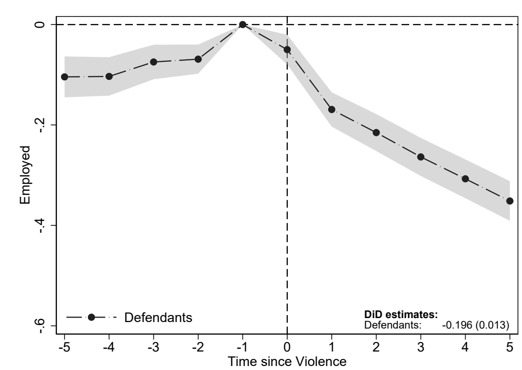



(d) Income

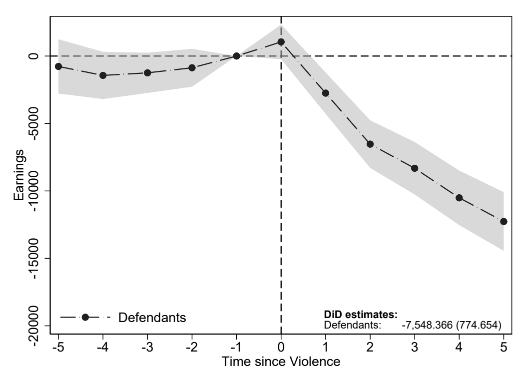



Panel III: Future Perpetrators as Counterfactual

(e) Employment




(f) Income

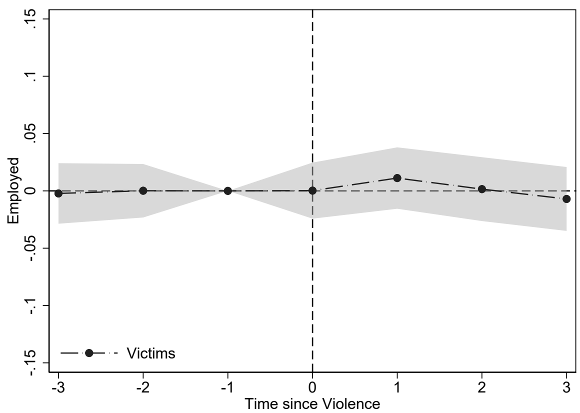



*Notes:* Each figure reports event study estimates of the impact of a violent incident between colleagues that results in a police report on perpetrator employment (left-hand side) or income (right-hand side). Panel I repeats the main estimates for all workplace crimes. Panel II reports results where we address the possibility of overfitting by dropping two of the pre-period years when matching. Panel III reports estimates using future perpetrators who attack a colleague in a year beyond the post period in a stacked event study as the control to identify effects 5 years before and 5 years after a violent incident. Specifically, we take perpetrators from 2014-2016 as the counterfactual for perpetrators from 2006-2008, thus treatment effects do not overlap in the post period. Employment and income are both measured at the end of the year, and income is measured in Euros. See Section 4.3 for more details.

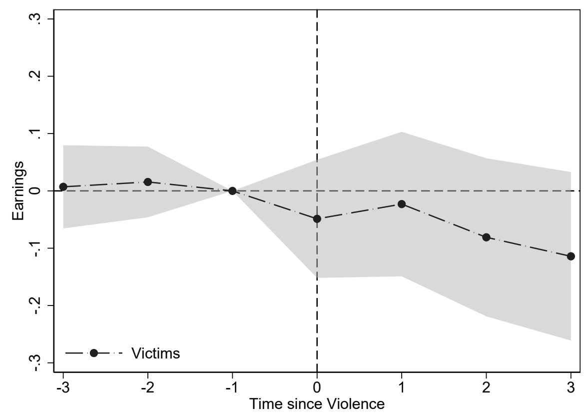
**Figure C.13:** Placebo Estimates of the Impact on Perpetrator Matching 5 Years Prior to Event

Panel I: Male-Female Violence Placebo Perpetrator Effects

(a) Employment

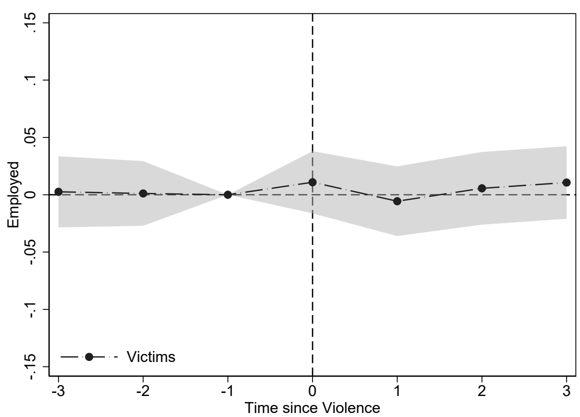



(b) Income

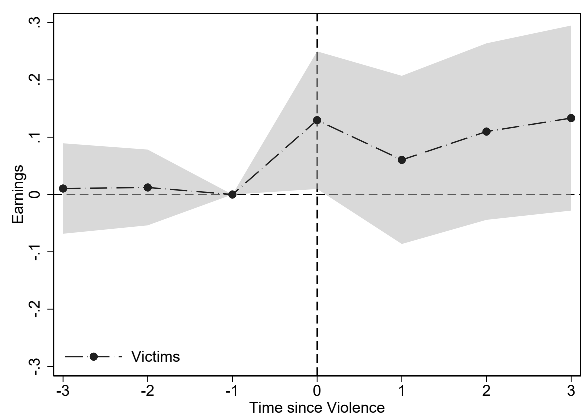



Panel II: Male-Male Violence Placebo Perpetrator Effects

(c) Employment



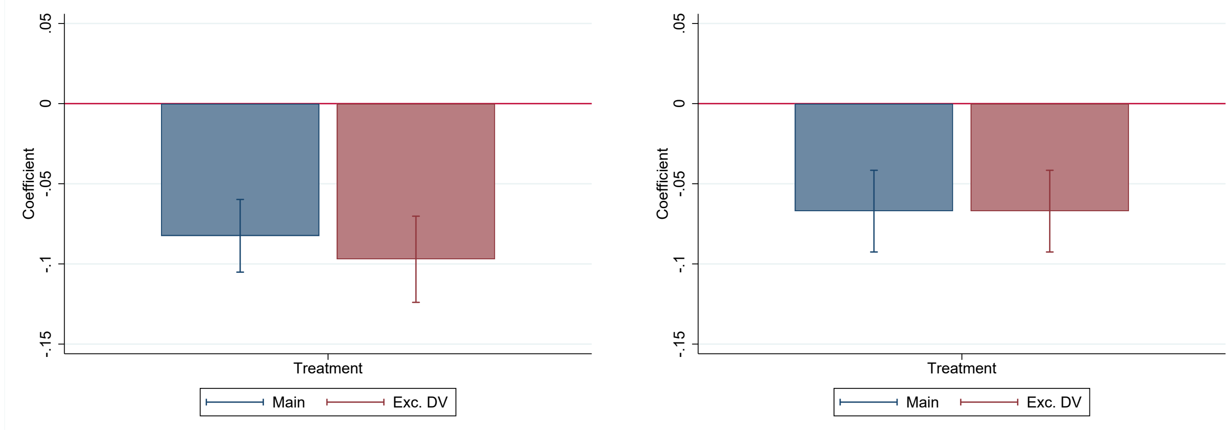

(d) Income




*Notes:* Panel I shows the impact of a placebo event 5 years prior to a male-female violent incident that results in a police report on the male future perpetrator's employment relative to his matched control in subfigure (a) and the male future perpetrator's income in subfigure (b). In Panel II we report the placebo results for male perpetrators of male-male crimes. Employment and income are both measured at the end of the year, and income is measured in Euros. The placebo exercise moves the "event" line 5 years prior to the actual violent event, redoes the nearest neighbor matching to find a counterfactual, and re-estimates equation 1 to calculate effects 3 years before and after this placebo event. For more details, see Section 4.3.

**Figure C.14:** DiD Employment Impacts Excluding Domestic Violence Cases from the Sample for Male-Female Violence Only

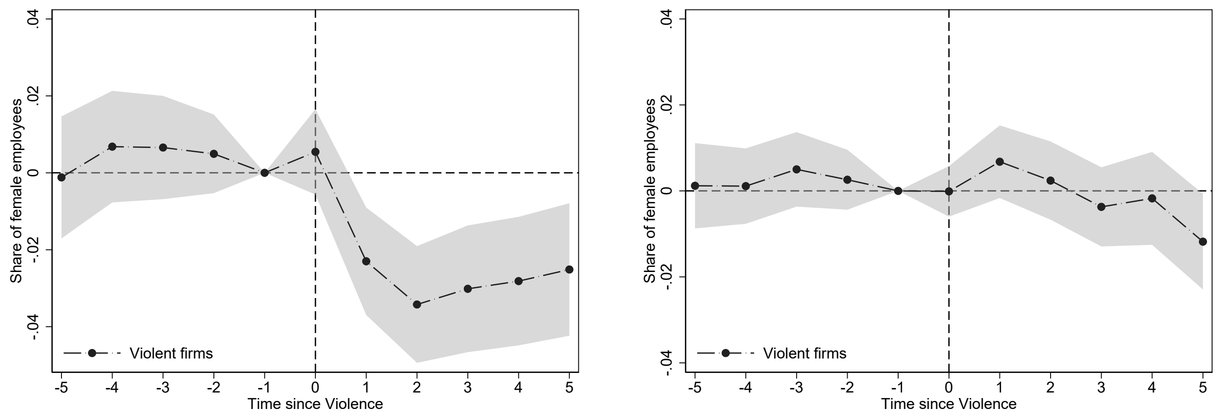
(a) Victim (b) Perpetrator




*Notes:* Figure (a) shows the DiD estimates with the dependent variable equal to employment for our main specification on the full sample (blue) and when we drop cases where the victim and perpetrator were cohabiting the year before or of the incident. Panel (b) gives the same for perpetrators.

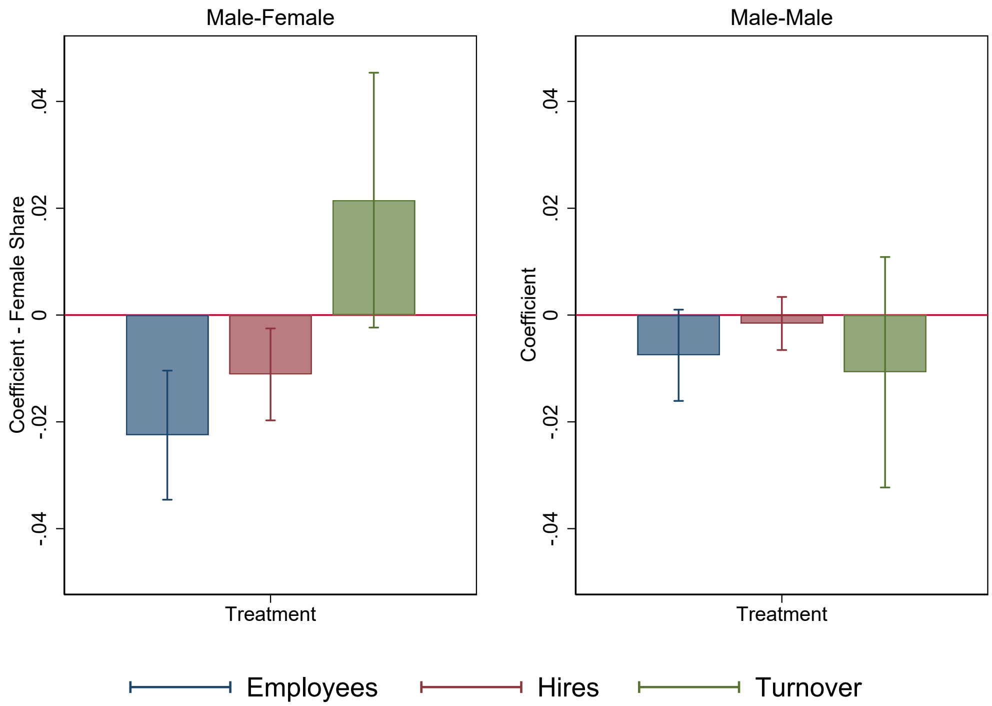
**Figure C.15:** Estimates of the Impact on Share Female Workers: Excluding Perpetrators and Victims

(a) Male-Female (b) Male-Male



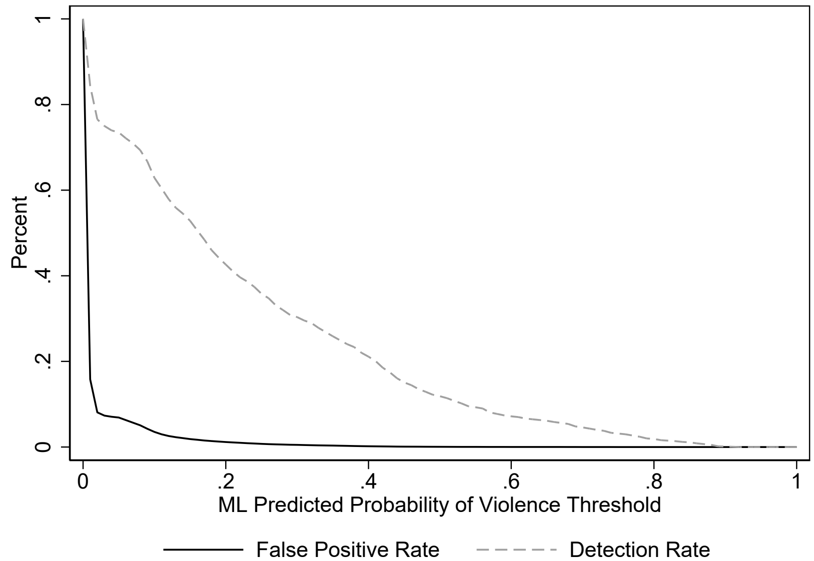

*Notes:* Figure (a) shows the impact of a violent incident against a colleague that results in a police report on the share of female workers for male-female crimes, and (b) shows impacts on share of female workers for male-male crimes. We exclude perpetrators and victims when calculating the share of female workers in a firm.

**Figure C.16:** Individual Components of the Drop in Share Female Employees




*Notes:* Figure reports DiD estimates of the impact of between colleague violence on the overall share of women in the firm (in blue, leftmost bar), the share of women amongst new hires (in red, middle bar), and female turnover in the firm (in green, rightmost bar). Impacts of male-female between colleague violence on these firm-level outcomes shown in the left panel while impacts for male-male violence shown in the right panel. Turnover is measured as the share of women amongst workers leaving the firm. Sample construction and data as defined in Sections 2 and 3.

**Figure C.17:** Machine Learning Trade-off Between False Positives and Identifying Perpetrators




*Notes:* Figure shows the trade-off at different predicted probability of violence cut-offs between identifying those who commit workplace violence ("True Positive Rate") and the percent of individuals who do not commit violence ("False Positive Rate"). This is for the holdout sample consisting of half of the data, with the other half of the data used to train the algorithm. See Appendix A and Athey *et al.* (2019) for more details.