VIOLENCE AGAINST WOMEN AT WORK*

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Abstract

In this paper, we link every police report in Finland to administrative data to identify violence between colleagues, and the economic consequences for victims, perpetrators, and firms. This new approach to observe when one colleague attacks another overcomes previous data constraints limiting evidence on this phenomenon to self-reported surveys that do not identify perpetrators. We document large, persistent labor market impacts of between-colleague violence on victims and perpetrators. Male perpetrators experience substantially weaker consequences after attacking female colleagues. Perpetrators' relative economic power in male-female violence partly explains this asymmetry. Turning to broader implications for firm recruitment and retention, we find that male-female violence causes a decline in the proportion of women at the firm, both because fewer new women are hired and current female employees leave. Management plays a key role in mediating the impacts on the wider workforce. Only male-managed firms lose women. Female-managed firms exhibit a key difference relative to male-managed firms: male perpetrators are less likely to remain employed after attacking their female colleagues.

JEL Codes: J16, J81, M54

*We thank six anonymous referees and the editor for their feedback which greatly improved this paper. We also thank seminar participants at ESPE, Georgetown University, IZA, Oxford University, NBER SI Personnel Economics, SOLE, Southern Methodist University, University of Antwerp, UCL, University of Nebraska-Lincoln England-Clark Conference, University of Oregon, University of Southern California, WSU, Washington University in St. Louis, and ViCE as well as Lori Beaman, Pascaline Dupas, Olle Folke, Hamish Low, Claudia Olivetti, Analisa Packham, and Johanna Rickne for helpful comments. We would like to thank the European Research Council for their generous funding (ERC Grant Number 948070).

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1 Introduction

The #MeToo movement demonstrated that violence between colleagues is an internationally relevant phenomenon. Yet there is little evidence on the impacts of workplace-related violence on perpetrators, victims, and the wider workforce, nor how these impacts may depend on power and gender differences between victims and perpetrators. The nascent economics literature on workplace sexual harassment has largely focused on less serious crimes, with a survey experiment featuring hypothetical cases of harassment suggesting potentially large costs to victims (Folke and Rickne, 2022). However, due to data limitations, this literature has thus far been unable to identify the consequences that perpetrators of violence face for assaulting a colleague nor the impacts on the broader firm and the role of management.

In this paper, we harness unique Finnish administrative data to analyze the impact of violence between colleagues 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.[1] Crucially, our data contains unique identifiers for both the victim and perpetrator. This allows us to identify violent incidents where both parties worked in the same plant (hereafter the "firm") at the time of an incident. While police reports will miss the full scope of violent incidents amongst colleagues as it is likely that most cases of (minor) violence go unreported, they provide an important step in understanding this phenomenon.

This new methodological approach to observe when one colleague attacks another and what happens subsequently underpins the analysis in this paper. It allows us to provide the first estimates of the economic consequences of realized events of violence between colleagues on both victims and perpetrators, to understand how the economic relationship between the two within the firm affects outcomes, and to explore the broader impacts of these events on firms, including the role of management. This innovation overcomes data constraints that restricted prior evidence on the topic from economics, psychology, and sociology to self-reports from surveys, which do not allow perpetrators to be identified.

We identify over 4,600 cases of violence between colleagues. If we take survey evidence that only 10% of assaults in Finland are reported to police seriously, then 3% of individuals in the labor

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

force are directly involved in colleague violence. Even more are impacted through the spillover effects that we will document on the broader workforce. 55% of incidents are assaults or petty assaults, and the remaining 45% are a mix of negligent bodily injury, menace, and other crimes. The vast majority (84%) of perpetrators are men, while victims are evenly split between male and female. 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 between-colleague crimes, where violence is more likely to occur between relative equals within the firm. 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 how colleague violence impacts victims' and perpetrators' outcomes. We begin with descriptive results, analyzing raw labor market outcomes in the years before and after the incident. A number of patterns immediately emerge. First, the earnings growth of both victims and perpetrators is robust and indistinguishable from all other workers in Finland in the years leading up to the event. This is not consistent with years of unreported abuse causing a deterioration in labor market outcomes long before the reported event we observe. Second, we observe a sharp drop in employment for both victims and perpetrators directly after an incident. There is an important asymmetry between male-female and male-male violence, with larger employment losses for female victims (compared with their male perpetrators) and smaller employment losses for male victims (compared with their male perpetrators).[2]

The precipitous employment losses following violence between colleagues in the raw data might not be caused by the violence itself. Some workers will always separate from their firm each year and poor labor market potential might cause an individual to commit violence against a colleague, or make a colleague more exposed to abuse. Such individuals may have experienced declines in employment even if they hadn't attacked (or been victimized by) a colleague. To mitigate these concerns, we employ a matched difference-in-differences design with individual fixed effects. Specifically, we compare the evolution of employment outcomes of affected workers

[2]We restrict our analysis of female perpetrated workplace violence to Appendix Section B since a) women rarely attack colleagues resulting in small sample sizes and b) in the majority of cases where a woman is recorded as the perpetrator, she is also recorded as a victim, thus these are not clear-cut cases of female perpetrated violence.

before and after an incident of workplace violence to observationally identical workers who were not affected.

This empirical strategy confirms the conclusions from our descriptive analysis. Following a violent incident, victims experience an immediate drop in employment that persists for at least five years following the incident. Perpetrators experience similarly large and persistent negative impacts. The dramatic asymmetry in the impact of colleague violence for male-female crimes versus male-male crimes remains. For male-male crimes results are as one might expect: perpetrators experience significantly greater negative repercussions than their victims. Employment rates fall by 12.3 percentage points for perpetrators and 7.5 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 7.1 percentage points on average in the five years following an incident, victim employment falls by 9.1 percentage points. This asymmetry is also apparent when we compare the impacts of between-colleague violence to non-colleague violence: perpetrators who attack female colleagues are significantly more likely to keep their jobs compared with observationally identical perpetrators who attack women who are not colleagues.

We examine power differentials between victim and perpetrator as one possible explanation for this asymmetry in the impacts of male-female versus male-male workplace violence. We interact the treatment of an attack by a colleague with an indicator for whether the perpetrator is a manager within the firm. We find that for male-female (male-male) crime, victims' employment rates fall by 7 (6.3) percentage points *more* when their perpetrator is a manager. However, perpetrators who are managers are 6 (13.7) percentage points *less* likely to be unemployed in the five years following an incident. Results are similar when using the difference in the within-firm income rank of perpetrators and victims as the measure of relative power, and when comparing to non-colleague violence. Thus, perpetrator power within the firm plays an important role in determining the impacts of violence between colleagues, and we show that this partially accounts for the comparatively smaller labor market impacts experienced by perpetrators of male-female violence where such power imbalances are more common. These results suggest that policies to support women moving up the occupational hierarchy may help mitigate these asymmetric impacts.

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.[3] This fall in the female share of employees only occurs in the case of male-female violence; there is no significant impact of male-male violence on the gender composition of employees. Moreover, there is no impact of non-colleague violence on the gender composition of the firm. 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.

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 and Van Reenen, 2007; Bloom *et al.*, 2013; Bender *et al.*, 2018; Bloom *et al.*, 2019; Bandiera *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 (Sarsons, 2017; Chakraborty and Serra, 2021). In particular, Egan, Matvos and Seru (2022) show that women found guilty of misconduct are more likely to be fired, and that this is entirely explained by the behavior of male-managed firms.

Motivated by these facts, we consider heterogeneity in the impacts of colleague 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. Perpetrators of workplace violence in female-managed firms are more likely to enter unemployment, although the gender composition of management does not significantly affect the direct victims' labor market outcomes. We find that it is both perpetrators losing their jobs, and female management more generally, that mitigates the consequences of colleague violence on the wider workforce.

Our findings contribute to two broad strands of literature. Most closely related is a small but growing literature showing that women disproportionately experience costly interactions with

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

peers at work (Bertrand and Hallock, 2001; Basu, 2003; Antecol and Cobb-Clark, 2006; Hersch, 2011; Sarsons, 2017). In particular, we complement Folke and Rickne (2022) who use survey data from Sweden to show that sexual harassment on the job is more likely for those in the gender minority, harassment is associated with higher turnover of victims, and that individuals presented with randomized hypothetical job offer vignettes in a survey setting are willing to give up approximately 10% of hypothetical 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 impacts of realized events of between-colleague violence for both male and female victims and their perpetrators, as well as the impacts on the broader firm. Given our unique data, we can link perpetrators and victims together, and examine how their relative economic standing mediates these impacts. Our results suggest that the patterns characterizing high-profile male-female assaults reported in recent years to the media hold true in general; namely, there are large impacts for victims and a relative lack of consequences for their male perpetrators, and this can be explained in part by perpetrators' relative power within the firm.

We additionally show that these events have impacts that extend well beyond the perpetrator and the victim to the broader firm and that management gender composition can play a role in mitigating these broader costs. These broader repercussions for peers within the firm complement a large literature documenting spillovers amongst peers in the workplace (Thornton and Thompson, 2001; Mas and Moretti, 2009; Cornelissen, Dustmann and Schönberg, 2017; Brune, Chyn and Kerwin, 2020; Nix, 2020), as well as in other contexts (Hoxby, 2000; Bayer, Hjalmarsson and Pozen, 2009; Black, Devereux and Salvanes, 2013).

Second, our paper makes an important contribution to the literature on firms and firm management on worker outcomes (Ichniowski, Shaw and Prennushi, 1995; Bertrand and Schoar, 2003; Bandiera, Barankay and Rasul, 2007; Gosnell, List and Metcalfe, 2020; Alan, Corekcioglu and Sutter, 2021). We show that extreme and toxic events between colleagues 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 and Serra (2021), and Egan, Matvos and Seru

(2022) described previously, along with Benson, Li and Shue (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.[4]

The paper is organized as follows. Section 2 describes the data, how we measure colleague violence, and provides descriptive statistics on the characteristics of victims, perpetrators, and violent firms. Section 3 presents impacts on victims and perpetrators. Section 4 presents impacts on firms. Section 5 explores the role of managers. Section 6 concludes.

2 Data and Descriptive Statistics

2.1 Data, Sample Construction, and Defining Workplace Violence

We use Finnish administrative data to study the impact of between-colleague violence on worker and firm outcomes. We obtained the universe of police reports filed between 2006-2019. 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 records, police reports are not.

The police reports include a unique case number for each reported crime, information on the day, month, year, and type of the offense, characteristics of the suspect (age, sex), characteristics of the victim (age, sex), and information on whether the case has been submitted to a prosecutor and whether the case has been solved. Note that there can be multiple charges associated with a given case (e.g. assault and menace). We focus on the total number of cases, not the total number of charges. The police also indicate which is the most serious charge for a given case, and this is the crime type we use when constructing descriptive statistics below.

Crucially, the police data includes unique national identification numbers for both the perpetrator and the victim when their identities are known. We use these unique national identifying

[4]Also related is Stoddard, Karpowitz and Preece (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.

numbers perfectly to link perpetrators and victims from the police data to the Finnish Linked Employer-Employee Data (FLEED) and FOLK data. FLEED is a population register data containing annual income, employment, and demographic characteristics. It covers every person alive in Finland aged 15-70, for the years 1988-2016. FOLK is identical but covers the years 2017-2019. This allows us to link everyone in the police data of working age to their demographic and employment information, including those who are outside the labor force, unemployed or self-employed. Note that all links between the police data and FLEED/FOLK are perfect (i.e. there is no way to get "incorrect" links in contrast to other contexts where fuzzier matching using names and other information is used and matches may not be perfect). FLEED/FOLK also includes unique plant and firm identifying numbers for everyone working in Finland. Thus, with this linked data, we are able to identify police reports between colleagues employed at the same establishment, a key innovation of this paper.[5] 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 construct the analysis sample in the following way. First, we merge police records from years 2006-2019 to FLEED/FOLK data using victim and perpetrator unique identifiers and identify between-colleague violence incidences. Formally, we classify an incident recorded in a police report as between-colleague violence if two conditions are met. First, and most importantly, both the victim and perpetrator must have worked at the same plant in December just preceding the incident (we only observe the place of employment in December of each year). This requires that both the victim's and the perpetrator's identities are known such that their unique identifiers are recorded in the police data.[6] Cases involving an unknown perpetrator (or unknown victim) will not be included. Second, we restrict our attention to cases with crime codes that capture any violent incident, including minor ones such as petty assaults and sexual harassment. We list all of the crime codes used to identify between-colleague violence in Appendix Section A.4.

We do not require that a case be sent on to courts for inclusion in our sample, nor can we verify the veracity of the reports. This is for two reasons. First, there is currently no link between the police data and court outcomes.[7] Second, evidence from criminology and sociology highlights

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

[6] Among all police reports, 75% include the identification of the perpetrators, i.e., the personal IDs of perpetrators.

[7] The link between the police data and court data is still ongoing. This linking is more complicated due to

significant case attrition between reporting and charging for gender-based violence that has little connection to the veracity of the case (Hester, 2006; Spohn and Tellis, 2012).[8] In a minority of cases, there are multiple perpetrators or multiple victims. We treat these as a single case for the purpose of counting the number of workplace violence incidents, include each perpetrator and victim separately for the individual analysis, and include the case only once in the firm analysis.

After merging the police data with our rich administrative data on individual labor market outcomes and creating the sample of between-colleague crimes, we construct the labor market trajectories of victims and perpetrators before and after an incident. For victims and perpetrators, we focus on two main outcomes: employment and income. Employment measures whether, and in which plant, an individual was working in the December of each calendar year. Income consists of all labor income reported to the tax authorities and measured in December of each year (salary and wage earnings, as well as any self-employment income, but excluding benefits). When estimating impacts on income, we estimate the impact of income measured in year t as a fraction of the average total labor market income in the 5 years before the event to address zeros. We additionally 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 allow us further to construct labor market outcomes of other workers employed at the same firm.

Measurement Discussion The limited previous literature on between-colleague 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 are key workplace environment factors with implications for individual and organizational performance (Cortina *et al.*, 2001; Estes and Wang, 2008; Geck *et al.*, 2017). Using administrative data to study this question 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

inconsistent inclusion of victim identifiers and lack of exact date of the crime.

[8] In addition, see anecdotal accounts from the #MeToo movement. For example, there was a 2015 police report for groping against Harvey Weinstein filed in 2015 by Ambra Gutierrez. She cooperated with police and wore a wire that confirmed her experience, yet the D.A. chose not to prosecute the case.

firm including workplace colleagues and new recruits, and the role of management.

Our measure of between-colleague violence captures the population of violent incidents that occur between 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. This is an important advantage of our data relative to an alternative scenario where one only observes violence occurring during working hours within the four walls of the firm. People can be assaulted by colleagues at off-premises holiday parties, when traveling for work, etc. Using our definition of between-colleague violence, all such incidents will be included. This means, however, that our measure will also capture domestic violence when partners work at the same firm. We find that fewer than 2% of domestic violence incidents occur between colleagues. Nonetheless, in Appendix Section C we discuss why we include these cases in our main analysis and also show that our results are robust to excluding all domestic violence cases from the sample.

The primary limitation of our measure is that we only 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 (European Institute for Crime Prevention & Control, 2009; EU Agency for Fundamental Rights, 2015). This estimate is for assaults in general; there does not exist a separate estimate for between-colleague assaults. Our measure, therefore, understates the true prevalence of colleague 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 (European Institute for Crime Prevention & Control, 2009; EU Agency for Fundamental Rights, 2015). This evidence suggests that the coverage of our measure might not differ significantly by gender, although we revisit this point in Section 6. If we take this 10% undercount at face value, then 3% of people in the labor force are directly involved in workplace violence as either a victim or perpetrator in Finland.[9] As we will document later, this is an underestimate of

[9]According to Table II, 9,300 individuals are involved in between colleague violence. If the true number of assaults is 10 times larger, this translates to 93,000 individuals. In 2010, 2,684,521 people were in the labor force. Assuming a majority of people are continually in the labor force over our sample period, then we find that 93000/2684251=3.4% of those in the labor force during this time were directly involved with colleague violence.

all those impacted by these events, given the spillovers on the broader workforce within the firm that we uncover. However, our results on the impact of between-colleague violence on economic outcomes may not perfectly extrapolate to unreported events due to selective reporting. We thus discuss the interpretation of our results in light of the incomplete reporting in Section 6 at the end of the paper.

2.2 Descriptive Statistics

Table I Panel A provides an overview of the types of crimes characterizing between-colleague violence in the police report data. In Table II, we report summary statistics for the characteristics of victims and perpetrators of between-colleague violence in the year before an incident. Table III gives the characteristics of firms in which perpetrators and victims are employed and compares their characteristics to the rest of firms in Finland.[10] Between 2006-2019, there were over 4,600 police reports of violence between colleagues in Finland. Table I Panel A shows that the majority of these incidents were one of four crime types: assault (36.1%), petty assault (18.7%), menace (17.2%), and negligent bodily injury (11.8%). Panel B shows the breakdown of crime types for non-colleague violence, but restricts to the four most common crime types for between-colleague violence for comparability (assault, petty assault, menace, and negligent bodily injury). The majority of crimes within these four categories are assault (54.7%), with a lower share consisting of negligent bodily injury (3.9%).

Individual Characteristics Table II Panel A reports individual characteristics for those involved in between-colleague violence while Panel B reports descriptive statistics for those involved in non-colleague violence, but restricted to male perpetrators. Panel A shows that approximately half of the victims in between-colleague violence are women. In contrast, 84% of perpetrators are men. Panel B shows that a larger share of victims of non-colleague violence, 59%, are men. Only 4.4% of perpetrators are repeat offenders of between-colleague violence.[11] Contrasting colleague with non-colleague violence in Panel B, victims and perpetrators of colleague violence have significantly greater labor market attachment than those involved in non-

[10] Note that these tables are constructed before imposing the estimation sample restrictions from Section 3.

[11] This could be indicative of either a lack of repeat offenders, but could also be due to other incidents going unreported. We show that our results are robust to looking only at first offenses in Appendix Figure E.7.

colleague violence. Only 33%-40% of those involved in non-colleague violence are employed in the year before a crime compared with 100% of those involved in between-colleague violence (by construction).

Table II reveals some key differences in the characteristics of between-colleague male-female and 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 €11,793 less per year than their male perpetrators, equivalent to 30% lower incomes. On average, the income gap between female victims and male perpetrators is almost twice as large as the average gender pay gap within the firm (see Table III). 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, which are more likely to occur between relative equals within the firm. We do not see the same large difference in average incomes for male perpetrators and female victims in the case of non-colleague violence. Table II Panel B highlights that the perpetrator and victim have almost identical earnings for both male-male and male-female non-colleague crimes. Thus, it seems that male-female between-colleague violence is uniquely characterized by the fact that women are attacked by an individual in a position of economic power.

The types of crimes that characterize male-female between-colleague violence are also more serious than those for male-male violence. This could be indicative either of differences in underlying severity by victim gender or a higher threshold for reporting in the case of male-female crimes. Table I shows that 63% of male-female incidents are assaults compared to 45% of male-male incidents. Beyond assault, the next most prevalent crime for male-male workplace violence is "negligent bodily injury", which is characterized 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.[12] Note that Table I reports statistics at the case level, and thus has more observations than Table II which collapses the data to the individual level within a year, i.e. if an individual commits multiple workplace crimes in the year, they are only included once in Table II.

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 main analysis, we do not single out cases with a female perpetrator for two reasons. First, women are rarely perpetrators. In only 16% 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 E.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 make it difficult to say anything conclusive about female-perpetrated crimes. We report estimates for these crimes in Appendix Section B.

Victim and Perpetrator Earnings Growth Prior to Violence A reasonable hypothesis before examining the data is that individuals who are attacked by a colleague may have been victimized for many years leading up to the event that they report to the police, resulting in a deterioration in earnings in the years preceding the event. Similarly, years of harassing a colleague might lead to a deterioration in earnings for perpetrators prior to the reported event. This is not what we find. In Figure I we graph earnings of victims and perpetrators compared to all workers in Finland of the same gender and of a similar age and education level.[13] We see that the pre-trends are identical, with no evidence of differences in earnings trends prior to the violent incident. This

[12]The greater severity of male-female crimes is also apparent if we look at the rate at which these types of crimes are incarcerated. 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%). These incarceration rates are for all cases with the relevant crime code, and do not restrict to cases of between-colleague violence.

[13]Appendix Figure E.4 gives employment growth in the years preceding the event (i.e. for years -5 to -1). There is robust employment growth in the years before a violent incident for both victims and perpetrators. Although this employment growth is somewhat mechanical (the victim and perpetrator must work together the year before the event for it to be violence between colleagues, so even if there is unemployment in prior years by year -1 they must be employed), the earnings growth is less mechanical. While moving from unemployment to employment will mechanically increase earnings, those who are continually employed could have experienced decreases in their earnings prior to the event.

growth in earnings leading up to the event provides a stark contrast to the abrupt deterioration in labor market outcomes that we will document after the violent incident.

Figure I also makes the difference in the economic relationship between victims and perpetrators of male-male versus male-female violence even clearer. For male-male violence, earnings levels and growth before the incident are indistinguishable between perpetrators and victims. For male-female violence, the women make significantly less on average than their male perpetrators. In Appendix Figure E.3, we report similar results to Figure I, but looking only at other workers within the firm. The dotted line again shows the earnings growth of victims (left-hand figure) and perpetrators (right-hand figure), but now the solid line shows the earnings growth of other workers in the same establishment of the same gender, and of a similar age and education level. We see that women who are attacked are relatively low-earning women within the establishment, even relative to other women of the same age and education. However, we still observe parallel trends in earnings growth before the event.

Our Data and Sexual Harassment Our data capture all cases of violence between two colleagues reported to the police. While this paper is concerned with between-colleague violence in general, we are also interested in the extent to which our data captures sexual harassment and "#MeToo" type cases. While our data is unlikely to include inappropriate sexually motivated comments, it will include groping and stalking incidents, which are commonly cited forms of sexual harassment, as well as rape and sexual violence.[14]

Groping incidents were categorized as assaults or petty assaults in the Finnish crime code prior to 2015, and our main analysis includes only events up to 2014 so that we can observe outcomes 5 years post-event. Assault/petty assaults make up the vast majority of our male-female cases as shown in Table I. Table I also shows that while the first two most common between-colleague crimes for male-female violence are assault and petty assault, the third most common category is "menace". This consists of "A person who points a weapon at another or otherwise threatens another with an offense under such circumstances that the person so threatened has reason to believe that the personal safety or property of himself or someone else is in serious danger". Stalking was added to the Finnish criminal code in January 2014. Prior to 2014, stalking

[14]Groping was a particularly common type of #MeToo case. For example, Andy Dick, Paul Rosenthal, Al Franken, Raul Bocanegra, Dean Westlake, and many more high-profile men were accused of groping during #MeToo.

involving threatening behavior would have fallen under the menace crime code (Hossa, 2012).[15]

Our data also includes incidents categorized as rape, aggravated rape, and sexual abuse (see Appendix Section A.4). However, the Finnish crime code had a very strict definition for rape and sexual violence over the period covered by our data. It did not, for example, include a consideration of consent and was rather based on the degree of force and physical violence perpetrated. This approach has been internationally criticized and experts have argued that it caused many acts of sexual violence not to be labeled as such, leading to perverse outcomes at other points of the criminal justice system.[16] For example, the official baseline report by the Council of Europe on the consistency of Finnish policy with its *Convention on Preventing and Combating Violence against Women and Domestic Violence* noted that the Finnish criminal definition of rape created "higher thresholds of evidentiary standards of physical resistance" and "that not all forms of sexual violence are criminalised [as such] in Finland" (Council of Europe, 2019). For this reason, and because our interest is in violence between colleagues more broadly, we do not restrict our analysis only to cases categorized by police as sexual violence and rape.

We also note that many anecdotal accounts of sexual harassment at work are characterized by men in positions of power taking advantage of their (relative) power to attack lower-ranked women with few repercussions. These power differences are already apparent in the descriptive results for male-female violence in Figure I, and we will investigate the important role of power dynamics in detail in Section 3.3.

Establishment Characteristics Turning to establishment characteristics, Table III shows that establishments in which colleague violence is reported do not appear to be negatively selected relative to all other establishments in Finland. They have higher wages and longer tenure on average. Establishments where male-female crimes take place also do not appear to be bad places to work for women based on observables. They have a higher female share overall, a higher share of female new hires, and similar gender pay gaps compared to male-male violent establishments,

[15]If stalking behavior is threatening, it would be classed as a menace both before and after 2014. See `https://poliisi.fi/en/stalking`. In some cases, stalking behavior could have also fallen under the coercion code prior to 2014.

[16]For example, Finland's supreme court ruled in 2018 that a man who sexually abused a 10-year-old girl could not be charged with rape as there was no evidence the crime had involved violence or that the child was overcome by fear (Boffey, 2019).

and approximately the same female share and gender pay gaps as all other establishments in Finland. The dimension along which violent and non-violent establishments differ the most is firm size, as violent establishments are significantly larger. This may be partly mechanical: in larger establishments, there are more potential combinations of colleagues between whom violence can occur. This means that even if the assault risk per worker is still the same, the probability an event occurs within a larger establishment is higher.

In Appendix Table D.1 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). Establishments 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 E.2). Mining and Quarry industries are more likely to experience male-male violence, but not more likely to experience male-female violence. However, the apparently higher rate of workplace violence in public sector firms may be an artifact of better reporting or support structures in those firms. While analysis of what causes reporting is beyond the scope of this paper, it is an important area for future research. Controlling for industry fixed effects, we see that establishments in which colleague violence occurs are slightly younger, less educated, and lower paying although the magnitudes of these coefficients are all small. We also find that establishments in which colleague violence occurs are less likely to be female-managed, especially in cases where male-female violence takes place. There is no statistically significant difference in turnover rates nor in gender pay gaps between violent and non-violent establishments.

3 Impacts on Victims and Perpetrators

3.1 Descriptive Labor Market Outcomes

Figure II gives the raw change in employment rates of perpetrators and victims the year after an incident relative to the year before. There is a large and immediate drop in employment at the exact point of the incident for perpetrators and victims of both male-female and male-male violence. The magnitude of this decline varies according to the gender of the victim. Female victims experience a 20 percentage point decline in their employment while their male perpetra-

tors experience a smaller decline in employment of just over 15 percentage points the year after the event compared to the year before. Male perpetrators of male-male violence experience a 24 percentage point decline in employment compared with an 18 percentage point decline for their victims. Note that these sudden declines follow consistent and robust increases in employment rates and earnings in the years leading up to the violent incident for both perpetrators and victims (see Figure I, and Appendix Figures E.4-E.5).

3.2 Estimated Labor Market Outcomes

The raw means depicted in Figure II suggest potentially large impacts of between-colleague violence and an important asymmetry in those impacts for male-female versus male-male events. The main concern with these descriptive results is that there may be some other factor that explains the immediate drops in employment following the violence. For example, many workers naturally separate from employment each year. Moreover, perpetrators might target economically vulnerable colleagues, or might themselves be struggling in the firm and lashing out as a result. The descriptive results above would fail to recover the true impacts of violence if declines in a victim's (or perpetrator's) employment directly after the event occur not because of the violence, but because the victim (or perpetrator) was going to be fired or quit that year in any case.

To mitigate this type of concern, we use a matched difference-in-differences design, comparing victims and perpetrators of workplace violence relative to a matched control observation (Goldschmidt and Schmieder, 2017; Aneja and Xu, 2022). This approach allows us to carefully compare the evolution of outcomes before and after the incident for treatment and control observations with similar characteristics.[17] Likewise, we will estimate the impact of workplace violence on firm outcomes relative to a matched control firm later in the paper.

Formally, we find a victim's (and perpetrator's) nearest neighbor match on the basis of their age, education level, gender, managerial status, employment and income history in the five years before the incident.[18] We restrict to victims and perpetrators that we can follow 5 years before

[17]We employ a stacked difference-in-difference design comparing never treated to treated individuals which addresses recent concerns in this literature. See Cengiz *et al.* (2019) for more details.

[18]In the matching, we control for age and log income linearly; and control for education level, gender, managerial status, and employment by fixed effects. When there are cases of zero income, we use log(income+1). Note that we only make this adjustment in the matching. When reporting impacts on income, we report impacts on future labor

until 5 years after the event. For firm outcomes, we find the nearest neighbor match on firm characteristics: firm size, turnover rate, industry, the average age of workers, average education of workers, share of new hires, and gender composition.[19] 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.[20]

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

$$Y_{ibt} = \sum_{j=-5, j\neq -1}^{5} \delta_j D_{ib,t-j} + \alpha_{ib} + \gamma_t + \omega_j + Age_{ib} * \beta_j + \epsilon_{ibt}, \quad (1)$$

Y_{ibt} represents the outcome of interest for victim (perpetrator) i in base-year sample b at time t.[21] b is the year in which the violent incident occurs. $D_{ib,t-j}$ is an indicator variable for the treatment (workplace violence) separately for each year j since the event. δ_j are the coefficients of interest, identifying the effects of the violent incident on victim, perpetrator, or firm outcomes relative to the matched counterfactual. We omit the year prior to the event ($j = -1$), which means that all estimates of δ_j are relative to the year before the incident. Additionally, we include individual-incident-year fixed effects (or firm-incident-year fixed effects when examining firm outcomes), α_{ib}, year fixed effects, γ_t, time since crime fixed effects, ω_j, and age at the time of the incident by time since event interactions, $Age_{ib} * \beta_j$.[22] Standard errors are clustered at the individual level (or firm level when examining firm outcomes).

We report the separate yearly effects δ_j for the 5 years after the incident or report overall difference-in-differences (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 (Goodman-

market income as a share of average income in the 5 years prior to the incident. For perpetrators, we also control for the number of cumulative crime records linearly.

[19] We find similar results for firm outcomes whether we use the individual match or the firm match.

[20] We don't restrict to same-firm matches as they are also treated given the colleague spillovers we will document.

[21] When we examine 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.

[22] People are different ages at base year, so this is not collinear with individual and time since event fixed effects. Our results are robust to adding incidence-year-specific time-since-event fixed effects.

Bacon, 2018; Sun and Abraham, 2020), i.e. this is a stacked DiD exercise as in Cengiz *et al.* (2019).

A key assumption for a causal interpretation of these results is that the employment of the victim (perpetrator) would have evolved along common trends as their matched control in the absence of one colleague attacking another. A similar assumption must hold for firms and their matched controls. Appendix Figures E.4 and E.5 show raw employment and income before and after the violent incident for victims and perpetrators, along with their matched controls. Prior to the 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 -1). Additionally, in Section 3.4 we present a placebo check showing that matched controls and treated individuals' employment do evolve similarly in the post period when we instead examine a period where no violence takes place, providing additional reassurance for our approach. We discuss remaining concerns for the validity of our estimates and how we address them in Section 3.4, after presenting the main estimates.

Results: Full Impacts of Between Colleague Violence Figure III gives the event study coefficients of interest from Equation 1 for victims and perpetrators with employment as the outcome of interest in the first two rows, and income in the last 2 rows. Figure IV reports the aggregated difference-in-differences results. In Figure III Panel I and II we see that the employment rate of victims of male-female violence falls approximately 8.2 percentage points in the first year after the incident relative to their matched controls, which grows to a 12.1 percentage point fall in employment by five years later. There are similarly large impacts on labor market incomes, with victims losing over 20% of their incomes relative to their average incomes before the incident (Figure III Panel III and IV). Perpetrators of male-female workplace violence also experience negative impacts on their employment and incomes.[23] The second and fourth rows of Figure III report event-study coefficients for victims and perpetrators of male-male violence. Victims see an

[23]In the five years after the incident, there is a 7.1 percentage point drop in perpetrator's employment and an 11.6% fall in income relative to the pre-violence baseline (see Table D.3).

immediate 7.1 percentage point drop in employment that grows to 11.7 percentage points 5 years later, and their incomes decrease by just under 20%. Perpetrators see an immediate employment decline of 14.3 percentage points which grows to 15.2 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 of between-colleague violence for victims and perpetrators across male-female and male-male incidents, consistent with the raw descriptive results. Figure IV succinctly summarizes this, showing the overall DiD impacts on the perpetrators and victims of male-female versus male-male crimes.[24] For male-male violence, perpetrators suffer a significantly greater labor market cost than their victims: their employment rates fall by over 12 percentage points in the five years following an incident, and this impact is significantly different than the 7.5 percentage point decline in employment for victims. This is the result one might expect. It seems reasonable 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 between perpetrators and victims. Indeed, if anything, perpetrators experience smaller labor market consequences compared to their victims. Overall, perpetrators' employment rates fall by 7.1 percentage points. This is smaller than, although not statistically distinguishable from, their female victims whose employment falls 9.1 percentage points. Panel (b) shows 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 also column (3) of Appendix Table D.4).[25]

In Appendix Figure E.8 we examine the impact of a violent incident on leaving the firm rather than employment. Leaving the firm occurs if the victim/perpetrator moves to unemployment, but also if they leave for a different firm but remain in employment. We find a similar asymmetry in the perpetrator impacts of between-colleague violence. Male-male perpetrators are just over 10

[24]Results are similar if we only estimate results 1 year or 3 years post, see Appendix Figure E.6

[25]Victims of male-female crimes experience a loss in income of €2,720 on average (see Appendix Table D.4). This is a sizable magnitude: column (3) of Table D.4 shows that female victims' incomes fall 19% on average compared to the pre-violence baseline.

percentage points less likely to be employed in the same firm in the five years following a violent incident. In comparison, perpetrators of male-female violence are just over 5 percentage points less likely to remain in the same firm relative to their matched control, almost half the size of male-male perpetrators. 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.

Results: Comparison to Non-Colleague Violence The estimates above focus on the impact of being attacked by a colleague relative to a counterfactual in which no violent event occurs. However, an equally interesting question is how the impact of being attacked by a colleague compares to the impact of being a victim of a similar crime, but with a perpetrator who is not a colleague.[26] Similarly for perpetrators. For firms, we might be interested in whether there is a difference in outcomes when an employed perpetrator attacks a colleague versus if they commit a crime against a non-colleague.

As shown in Table II, individuals involved in crimes with non-colleagues have very different characteristics to those involved in between-colleague crimes. Thus, to better isolate the impact of crime between colleagues versus crimes between non-colleagues, we carefully compare similar individuals across the two groups. Specifically, we match exactly on employment status in the year preceding the crime, crime type, and victim and perpetrator gender. We then identify an individual's nearest neighbor on the basis of their age, education, and employment/income outcomes in the five years preceding an incident.[27] In other words, we carefully compare outcomes of victims and perpetrators before versus after violence, comparing observationally identical people where the only difference is that the perpetrator (or victim) attacked (or was attacked) by someone within versus outside of their workplace.

The results of this exercise are presented in Figure V, which gives the matched difference-in-differences estimates for victims and perpetrators of male-female and male-male violence with the non-colleague violence counterfactual. The limited employment losses for perpetrators of male-

[26] Related is a small literature showing negative impacts of crime in general on victims in other contexts (Johnston, Shields and Suziedelyte, 2018; Currie, Mueller-Smith and Rossin-Slater, 2018; Koppensteiner and Menezes, 2021; Bindler and Ketel, 2022).

[27] For this exercise we restrict our analysis to the four most frequent crime codes in our data (Table I) to allow an exact match on crime type.

female workplace violence are even more pronounced when one considers the counterfactual of violence between non-colleagues. For male-male violence, we see no significant differences between the employment losses for men who attack colleagues compared with observationally equivalent individuals who commit the same category of crime with a non-colleague, and the same is true for male victims. These results suggest that male-male violence has similar impacts whether it occurs within or outside of the firm.

In contrast, for male-female violence, victims of between-colleague violence suffer 2.3 percentage points *greater* employment losses compared to women who are observationally equivalent and victims of the same crime but where the perpetrator was not a colleague (p-value=0.122). In contrast, perpetrators of colleague violence who attack women suffer significantly *smaller* employment losses. Perpetrators are 2.5 percentage points more likely to keep their jobs over the five years following the incident when they attack a workplace colleague compared to an observationally equivalent individual who commits a crime against a non-colleague (p-value=0.011). This is a striking finding given that it is plausible that violence against a colleague is more likely to be public to decision-makers within the firm.

3.3 The Role of Power Discrepancies Between Victim and Perpetrator

A key takeaway from the descriptive results and both counterfactuals is that men who attack female colleagues are comparatively insulated from employment costs compared to their victims, men who attack male colleagues, and men who attack women outside the firm. In this section, we explore one possible explanation for this pattern: the economic standing of perpetrators relative to victims within the firm. Anecdotal evidence from high-profile media accounts of male-female violence suggests that inequality between victims and perpetrators, and being attacked by an individual in a position of power, is especially problematic for victims. When describing the aftermath of her assault at the hands of Harvey Weinstein to her New York Times editorial, 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).[28] This was a common theme for female victims attacked by male

[28]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.

colleagues, even when the perpetrators were not as famous and powerful as Harvey Weinstein.[29] 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.

Power Differentials: Main Results In Table IV we consider heterogeneity in the impacts of workplace violence by the economic standing of victims relative to perpetrators within the firm.[30] 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 IV.[31] 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 the results of this second exercise in columns (2) and (4) of Table IV.

Table IV shows that the economic power of perpetrators matters for the consequences of both male-female and male-male violence. In both cases, victims face greater employment losses 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 7 (6.3) percentage points *more* when their perpetrator is a manager. However, perpetrators who are managers are 6 (13.7) 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

[29]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).

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

[31]This exercise uses the coarse occupation variable in the data, and whether this variable indicates a managerial role for the perpetrator.

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).

Power Differentials When Using the Non-Workplace Violence Counterfactual Next, we examine if the power differential explanation also holds true when we use the counterfactual of non-workplace violence. We report results in Appendix Table D.7 that replicate Table IV but replace the counterfactual of no violence with the counterfactual of non-workplace violence. We find that it is not simply being higher status in society that matters, but rather being high status when the colleague is also within the firm that matters for how these events impact employment.

Specifically, for male-female (male-male) crime, victims' employment rates fall by 4 (3.3) percentage points *more* when their perpetrator is a manager compared with non-workplace victims whose perpetrators are also managers, but not in the same firm. However, perpetrators who are managers are 4.1 (7.2) percentage points *less* likely to be unemployed in the five years following an incident compared with non-workplace perpetrators.

Gender, Power Differentials, or Both? The previous sections showed that for *both* male and female victims, having a manager attack them leads to much worse outcomes, and in *both* male-female and male-male violence, perpetrators who are managers or make significantly more than their victims are relatively insulated from unemployment. In this section, we explore what this means for our interpretation of the asymmetry in impacts across male-female and male-male crimes documented in Figure IV.

The descriptive statistics in Section 2.2 showed that female victims of between-colleague violence are lower-status in the firm and are more likely to be victims of more economically powerful men. This suggests an important composition difference between male-female and male-male between-colleague violence. Figure VI formally captures this point. It reports estimates from three linear probability models where the dependent variable is: (a) the managerial status of the perpetrator; (b) whether the perpetrator is in the top 20% of earners in the firm; (c) whether the perpetrator is in the bottom 20% of earners in the firm. The independent variable is the gender

of the victim, with controls for the crime type. The red bar indicates that female victims are significantly more likely to have a perpetrator who is a manager, and the blue middle bar indicates she is significantly more likely to be attacked by a manager in the top 20%. The green bar shows that female victims are significantly less likely to be attacked by a perpetrator in the bottom 20%.

In Appendix Figure E.9 we show female victims are more likely to be in the bottom 20% in terms of income in the workplace compared with male victims. This could be due to women being of lower status on average in the firm. To explore this possibility, we additionally estimate the impact of a victim being female on the within-firm within-gender income rank of the victim, constructing an indicator for whether the woman is in the bottom 20% of female earners within the firm. We still find a significant negative relationship between the within-gender income rank of victims and whether the victim is a woman. Thus, compared to male victims, female victims are relatively low earners within the firm compared to all workers and their gender peers.[32] Thus, while power differentials matter for both male and female victims (and also for perpetrators who attack men or women), women are more likely to be attacked by someone in power and be lower-earning within the workplace.

We analyze the degree to which these economic power imbalances can "explain" the gaps in perpetrator outcomes for male-female versus male-male violence in Table V. To do so we pool observations from male-female and male-male violence and interact the treatment of between-colleague violence with the gender of the victim and either the income or managerial gap between victims and perpetrators. We estimate:

$$Y_{ibt} = \delta D_{ibt} + \beta D_{ibt} \times \text{Female Victim}_i + \eta D_{ibt} \times \text{Power}_i + \alpha_{ib} + \gamma_t^g + \omega_j^g + Age_{ib} * \beta_j + \epsilon_{ibt} \quad (2)$$

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 between-colleague after the incident occurs. We again include individual-incident-year fixed effects, α_{ib}, year fixed effects, γ_t, time-since-event fixed effects, ω_j and age controls. We allow the year and time-since-event year fixed effects to vary by gender since women have different earnings profiles over time than men. Power is either a dummy taking value 1 if the perpetrator is a manager or is

[32]This is also consistent with the fact that female victims on average have lower tenure in the firm than male victims.

equal to the income rank gap between victim and perpetrator.

Table V gives the results. For victims, the coefficient on the interaction between the treatment and a victim's gender is negative but not significantly so. Consistent with Figure IV, there is no statistically significant gender difference in victim impacts in the pooled regression, even before controlling for power asymmetries in the relationship between victim and perpetrators (p-value=0.383). However, as shown in Figure IV, there is a significant difference in the employment consequences for male perpetrators who attack female versus male colleagues prior to controlling for power differences, with men who attack women 5.3 percentage points more likely to keep their jobs than men who attack men. However, the magnitude of the interaction between the treatment and whether a victim is female reduces by 32.4% (although it remains significant) once we control for the power imbalance between victim and perpetrator and whether the crime is an assault in column (7). Thus, the smaller employment losses for male perpetrators with female victims in Figure IV are partly (but not fully) due to a composition effect, i.e. power imbalances are more common for male-female crimes than for male-male ones, and these imbalances play a key role in mediating the impacts of workplace violence. Note that Columns (2)-(7) show that perpetrators are more likely to enter unemployment if they assault a colleague, consistent with more severe crimes leading to more severe consequences in general. We additionally show results are robust to including controls for the age gap between perpetrator and victim in Appendix Table D.6.

Together the results from this section show that female victims are systematically attacked by higher-status male colleagues compared with male victims. And this combination: those in power being more insulated from employment effects, and also men in power attacking lower-ranked and less powerful female colleagues to attack, seems to play a substantial role in explaining the asymmetry in Figure IV. These findings are crucial in terms of the economic and policy implications of this research. They suggest that policies that support women moving up the occupational hierarchy while also holding managers and higher-earning men accountable for the mistreatment of subordinates might both be key factors in mitigating the negative impacts of these events on women.

3.4 Eliminating Other Possible Explanations

The immediate, large, and discontinuous changes in victim 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, the fact that these differences persist for at least 5 years after the incident, and the lack of pre-trends showing deterioration in outcomes ahead of the event (see also Section 2.2) rules out many alternative explanations for our findings.

However, to rule out additional alternative explanations, in Appendix Section C we provide details of a series of robustness exercises which can be summarized as follows. First, we employ a fuzzier matching approach and show that pre-trends remain flat and the effect sizes are similar and size. Second, we use a different identification strategy instead of the matched difference-in-differences, namely, we use future victims as counterfactuals for current victims (and similarly for perpetrators) and find even larger effects. Third, we implement a placebo check where we estimate impacts on victims 5 years prior to workplace violence. We expect this exercise to show no impacts, and that is precisely what we find. Together, these additional checks and placebo tests help to reassure that our main results capture the true consequences of violence between colleagues and are not confounded by alternative explanations. We refer the interested reader to Appendix Section C for more details.

4 Impacts on the Firm

Does between-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 D.8). 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). Alternatively, hiring rates could be affected. As the majority of perpetrators 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.[33] 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 VII (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 over 2 percentage points by five years after the incident. Figure VII (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 in the four years post-violence (and equivalently, no significant impact on the share of men employed).

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. Figure VIII shows that both dimensions are relevant.[34] The proportion of women amongst firm leavers after male-female violence increases (p-value=0.148), while the share of women amongst new hires falls (p-value = 0.003).[35] While our research design removes any time-invariant differences across firms, including time-invariant hiring practices, this is interesting given that firms where male-female violence takes place did not avoid hiring women before the incident (Table III). 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.

It is interesting to explore whether the decline in the female share is driven by the presence of a perpetrator of violent crimes in general, who might exhibit a toxic workplace presence, or whether these spillovers on the broader workforce are linked to the workplace nature of between-colleague violence itself. We explore this in a placebo test in Figure IX, where we estimate the impact of a violent crime where the perpetrator is employed by the firm, but the victim is not,

[33]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.

[34]See Appendix Figure E.17 for the equivalent results for male-male crimes. As expected the figure shows no significant effects on any dimensions for male-male violence.

[35]New hires are workers who we observe in the plant in the December of that given year, who were not employed by the plant in December of the preceding year.

on the share of female employees 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. Since we find no spillovers of non-colleague violence in the broader workforce, for the remainder of the paper which focuses on understanding these spillovers, we do not consider the alternative counterfactual of non-colleague violence.

4.1 Interpreting 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, albeit imperfectly. Existing employee networks are an obvious way in which potential future hires could find out about and avoid workplaces where one employee attacked another through the "whisper network".[36] 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, Ross and Topa, 2008; Beaman and Magruder, 2012; Brown, Setren and Topa, 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 of higher quality through referrals from existing highly productive workers.

To test whether within-network hiring decreases following a violent incident, we follow Hensvik and Skans (2016) and construct the set of colleagues that each employee of the firm worked with in some previous employment relationship at some other firm in the 10 years prior to the incident year. We restrict to plants with 500 workers or less to make it more likely that employees in co-working networks actually know each other. We report event study estimates of the impact of between-colleague violence on the share of new hires that come from within this

[36]The supply-side response 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 meant to provide information to other women about bad firms and bad actors so that women can avoid them.

network relative to a violent firm's matched control observation in Appendix Figure E.18. We find a precise zero impact of a violent incident between colleagues on the amount of within-network hiring done by the violent firm. However, whisper networks about harassers are generally assumed to function primarily between women. Thus, in Panel (b) of Appendix Figure E.18 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.

While these results are suggestive that the fall in the female share is not purely driven by supply-side factors, they are not conclusive. The share of new hires who are past colleagues of current employees is quite low (Appendix Figure E.19) limiting the power of our test. Further, we cannot observe friend groups or other informal networks that may be even more effective channels for whisper networks. Last, some cases may become public when they reach courts, mitigating the importance of whisper networks, something we cannot observe in police data. Thus, more evidence is necessary in order to fully understand whether changes in the behavior of applicants or firms drive the fall in female hires following an incident of between-colleague violence.

5 The Importance 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 and Van Reenen, 2007; Bloom *et al.*, 2013; Bandiera *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 (Sarsons, 2017; Benson, Li and Shue, 2021; Chakraborty and Serra, 2021).[37] Most closely related to this paper, Egan, Matvos and Seru (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

[37] See also the literature on how female leadership impacts policy (Ford and Pande, 2011; Bertrand, Black, Jensen and Lleras-Muney, 2019).

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 27% of the top 20% of earners in firms where male-female violence occurs, with a standard deviation of 35%. 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 X 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 relative to their matched control, 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 VII we estimate the role of male versus female management in a stacked DiD 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=0.001), such that the gender composition of the firm remains unchanged.

5.1 Differences in Female-Managed Firms

How does female management mediate the impact of male-female violence on the gender composition of the firm? A surprising finding of Section 3 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.

We first analyze the relationship between female management and the individual labor market impacts of violence on victims and perpetrators. Table VI gives the stacked DiD coefficients on the treatment variable and its interaction with a dummy variable for whether the firm is female-managed, where the counterfactual is given by individuals' matched control observations. 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 male-female (male-male) violence, perpetrators in female-managed firms have an approximately 5.5 (8.4) percentage point greater reduction in employment compared to their matched control and relative to male-managed firms.[38]

Thus, female-managed firms appear to be less accommodating of perpetrators of workplace violence *in general*, and not just for male-female violence. The major difference is that perpetrators of male-female violence in male-managed firms are significantly less likely to fall into unemployment compared with perpetrators of male-male violence. The more negative consequences for perpetrators in female-managed firms could be due to female managers being more likely to fire perpetrators. Another possible explanation is that firms with higher shares of women in their managerial force have relational atmospheres in which it is not tolerable for one colleague to assault another, with perpetrators more likely to choose to leave after an event as a result (e.g., because their relationships with their coworkers deteriorate, their relationships with their managers deteriorate, and/or their promotion possibilities deteriorate).

In Table VII, we jointly examine the impact of perpetrators losing their job and of female management on the share of women employed in the firm following male-female violence. While we showed that perpetrators are more likely to separate from firms with higher shares of female managers following a violent event which means that the results in this table should be interpreted as suggestive, we still view this exercise as informative. To capture perpetrator job loss,

[38]We also find that perpetrators are 4.5 (1.5) percentage points more likely to become not employed under female management following male-male (male-female) between-colleague violence if we use a dummy for whether the manager of the plant is female as opposed to the definition of management based on top earners used in our main specification.

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=0.006), although this does not fully negate the full impact of the violence. Interestingly, column (3) show that while female-managed firms where the perpetrator remains employed still have different outcomes to male-managed firms (p-value=0.081), the perpetrator entering unemployment in either male- or female-managed firms seems to mitigate negative effects on the broader workforce.

Robustness of Management Results Female-managed firms may be different on a host of characteristics beyond the gender composition of management. First, we investigate other possible correlates with our measure for female management and control for them in the main results. Specifically, we interact the treatment of between-colleague violence with dummy variables indicating above or below median firm size, median earnings, median age, median tenure, and median share female within the firm. Our results are virtually unchanged after including these controls. It is still the case that the perpetrator is much more likely to enter unemployment following violence in female-managed firms (Appendix Table D.9 columns (1), (2), (5), and (6)). We also find that our results on the impact of female management on the share female within the firm are virtually unchanged (Table VII columns (4)-(6)).

In addition, we showed that perpetrators of male-female violence are more likely to be managers themselves. One explanation for our results is that male managers may also be perpetrators and choose not to fire themselves. To explore if this is the operative channel for the female management impacts on perpetrator employment, we replicate Table VI but exclude cases where the perpetrator was also a manager. We report results in Appendix Table D.9 columns (3)-(4) for male-female cases and (7)-(8) for male-male cases. We find that even in these cases, female management is associated with a higher likelihood that perpetrators enter unemployment, suggesting that this is not the only channel for these results.

6 Conclusion

In this paper, we estimated the impacts of realized violence between colleagues on victims, perpetrators, and the broader firm. We find that one colleague assaulting another has large negative economic impacts on victims and perpetrators. However, male perpetrators of male-female violence experience less severe repercussions compared with perpetrators of male-male violence. Our results show that relative economic power within the firm plays a key role in insulating male perpetrators of violence against women at work. Male-female between-colleague 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.

Our results have a number of implications. First, female victims of workplace violence have few economic incentives to report violence at work. A major issue in preventing harassment at work is that victims rarely report the problem (Magley, 2002).[39] Understanding who reports, and how reporting can be increased, is a key area for future research (Boudreau *et al.*, 2023). 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 between-colleague violence, given just 10% of physical assaults are reported to the police in Finland, with lower reporting rates for crimes considered less serious by the victim (European Institute for Crime Prevention & Control, 2009; EU Agency for Fundamental Rights, 2015). 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. We might not expect to see quite as large labor market impacts on victims, perpetrators, and the firm from less severe abuse by

[39] "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)

colleagues. However, our effect sizes are equivalent in size to other important economic shocks that have motivated large literatures. For example, in Finland, an exogenous job loss reduces employment over the next six years by 10.9 percentage points (Kaila, Nix and Riukula, 2022). This is only slightly larger than the employment effect for women of being victimized by a colleague from 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, 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, consistent with the descriptive facts documented in Folke and Rickne (2022).

Last, our results lead to the question of what can be done to mitigate the costs of these events. The results from this paper provide one optimistic takeaway: female-managed firms 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 force the perpetrators of these crimes into unemployment. 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, Matvos and Seru (2022) and Cullen and Perez-Truglia (2019).

Regardless, these results suggest 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. Our results demonstrate that this is not done as frequently amongst male-managed firms. Such lack of consequences not only benefits perpetrators at the cost of their victims but is also costly to women in general, as they are less likely to be employed within these relatively

high-paying firms in the future. However, our analysis cannot predict if firing perpetrators would increase the profits of these firms. If the perpetrators are highly productive workers, then there may be high economic benefits to retaining them, presenting a difficult decision for managers. Such decisions would likely also depend on a notion of fairness and justice which is well beyond the scope of our analysis.

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 the 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 the growing literature on the importance of who is in management. 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 affected 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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Table I: Crime Types

Crime types	All		Male-Female		Male-Male	
	Number	Percent	Number	Percent	Number	Percent
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Between-Colleague Violence						
Assault	1678	36.1	820	41.9	625	31.5
Petty assault	868	18.7	406	20.7	275	13.8
Menace	799	17.2	338	17.3	377	19.0
Negligent bodily injury	549	11.8	93	4.7	393	19.8
Others	756	16.3	301	15.4	317	16.0
Observations	4650		1958		1987	
Panel B: Non-Colleague Violence, Male Perpetrated Incidents						
Assault	233236	54.7	84899	48.7	148337	58.9
Petty assault	90275	21.2	40208	23.1	50067	19.9
Menace	86165	20.2	41690	23.9	44475	17.7
Negligent bodily injury	16443	3.9	7518	4.3	8925	3.5
Observations	426119		174315		251804	

Notes: The table reports the different types of crimes committed between colleagues for the full sample, the male-female violence sample, and the male-male violence sample in Panel A, as indicated. Other contains a subset of the 57 other possible crime codes that pertain to violent crimes in Finland. See Appendix Section A for the full list of crimes, which also contains additional details on sample selection. Panel B reports the same but for non-colleague crimes, i.e. crimes between two people who do not also work together. For non-colleague crimes, we restrict to the four most common types of between-colleague crime types: assault, petty assault, menace, and negligent bodily injury. In Table II we collapse the data to the individual level within a 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 II has fewer observations than this table, since in this table multiple crimes in the same year are all included as separate observations.

Table II: Sample Means for Perpetrators and Victims

	All		Male-Female		Male-Male	
	Perpetrator (1)	Victim (2)	Perpetrator (3)	Victim (4)	Perpetrator (5)	Victim (6)
Panel A: Between-Colleague Violence						
Share Male	0.84	0.53	1.00	0.00	1.00	1.00
Age	39.44	37.93	40.71	37.89	38.38	37.43
Share college	0.16	0.14	0.16	0.17	0.14	0.09
Share high school	0.56	0.59	0.56	0.61	0.57	0.59
Share dropouts	0.28	0.27	0.28	0.22	0.29	0.31
Employment	1.00	1.00	1.00	1.00	1.00	1.00
Earnings	38746	32331	38923	27130	40615	36993
Current employer tenure	5.14	4.54	5.16	4.33	5.37	4.82
Overall tenure	10.21	9.90	10.45	10.11	9.87	9.50
Share manager	0.13	0.06	0.16	0.05	0.12	0.07
Share professionals	0.21	0.18	0.23	0.21	0.20	0.14
Share clerical/service	0.17	0.22	0.17	0.28	0.11	0.12
Prior crimes	2.03	1.29	2.25	0.78	2.15	1.66
Observations	4024	3841	1584	1555	1784	1662
Panel B: Non-Colleague Violence, Male Perpetrated Incidents						
Share Male	1.00	0.59	1.00	0.00	1.00	1.00
Age	33.03	33.95	36.30	35.92	30.72	32.50
Share college	0.04	0.07	0.05	0.10	0.03	0.05
Share high school	0.36	0.42	0.40	0.45	0.34	0.39
Share dropouts	0.60	0.51	0.55	0.45	0.63	0.56
Employment	0.35	0.40	0.37	0.43	0.33	0.39
Earnings	10197	11301	11340	10965	9406	11533
Overall tenure	9.22	9.22	10.04	10.20	8.65	8.54
Share manager	0.01	0.01	0.01	0.01	0.01	0.01
Share professionals	0.05	0.09	0.06	0.12	0.04	0.07
Share clerical/service	0.05	0.13	0.05	0.20	0.06	0.08
Prior crimes	9.65	3.82	9.42	2.18	9.81	4.95
Observations	426119	426119	174315	174315	251804	251804

Notes: Panel A reports sample means for all perpetrators and victims of colleague violence. Panel B reports the same for non-colleague violence. Data is from police reports linked to FLEED register data. 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 an individual who commits multiple workplace crimes in the same year only appears once, explaining the smaller number of observations compared with Table I. Income consists of all labor market earnings (the sum of wage, salary, and self-employment earnings) and is deflated to 2013 Euros. Prior crimes consist of all prior police reports excluding traffic crimes and does not condition on arrests or convictions. Share college indicates those with a bachelor's degree or higher. Note that the majority of those who go on to college in Finland also receive a master's degree. High school consists of those who graduate with either a vocational-secondary or academic-secondary degree as their highest educational degree.

Table III: Establishment Summary Statistics

	Violent Firms			Other Firms
	All	Male-Female	Male-Male	
	(1)	(2)	(3)	(4)
Panel A: Characteristics of the Workforce				
Median number of workers	32	25	40	4
Average age of workers	39.89	39.95	39.93	41.03
Average wages	29029.21	26838.09	32385.40	25604.02
Share college	0.13	0.14	0.11	0.14
Share high school	0.57	0.57	0.58	0.56
Share dropouts	0.30	0.29	0.31	0.29
Average tenure	7.41	7.13	8.13	7.10
Turnover rate	0.23	0.25	0.23	0.31
Share of new hires	0.33	0.35	0.31	0.31
Panel B: Gender Characteristics of the Firm				
Share of women	0.37	0.46	0.21	0.44
Female turnover rate	0.10	0.12	0.07	0.16
Share of new hires	0.33	0.35	0.31	0.31
Share of female new hires	0.14	0.18	0.08	0.15
Average male earnings	31779	30257	33905	28758
Average female earnings	25553	23338	29012	22945
Average gender pay gap (male-female)	6680	6919	6726	6796
Median gender pay gap	5667	5455	6194	4469
Share female managers	0.22	0.26	0.14	0.30
Observations	3154	1520	1357	2632327

Notes: Table reports sample means (unless otherwise indicated) for all establishments that experience between colleague violence (column 1), as well as establishments that experience male-female violence (column 2) and male-male violence(column 3). Sample means for all other establishments 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 establishment outcomes. We also note that the smaller number of observations compared with Table II is due to the fact that a single establishment can have multiple cases of workplace violence, but we only enter the establishment once per year for this table. New hires are workers who we observe in the plant in the December of that given year, who were not employed by the plant in December of the preceding year. Turnover rate is the share of employees leaving each year as a fraction of total employees in the plant.

Table IV: The Role of Power Discrepancies between Victim and Perpetrator

Dependent Variable:	Victim Employment		Perpetrator Employment	
	(1)	(2)	(3)	(4)
Panel A: Male-Female				
Treatment*Perp is Manager	-0.070		0.060	
	(0.034)		(0.022)	
Treatment*Income Gap		-0.037		0.086
		(0.019)		(0.019)
Treatment	-0.085	-0.073	-0.077	-0.114
	(0.013)	(0.015)	(0.013)	(0.017)
Observations	22,861	22,861	23,253	23,253
Dependent variable mean	0.854	0.854	0.867	0.867
Panel B: Male-Male				
Treatment*Perp is Manager	-0.063		0.137	
	(0.043)		(0.023)	
Treatment*Income Gap		-0.066		0.125
		(0.021)		(0.019)
Treatment	-0.070	-0.049	-0.134	-0.172
	(0.012)	(0.014)	(0.013)	(0.016)
Observations	23,076	23,076	23,374	23,374
Dependent variable mean	0.857	0.857	0.861	0.861
Year fixed effects	✓	✓	✓	✓
Time since crime fixed effects	✓	✓	✓	✓
Individual-incident-year fixed effects	✓	✓	✓	✓
Age x time since crime	✓	✓	✓	✓

Notes: Table reports difference-in-differences 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. Standard errors are clustered at the individual level.

Table V: Pooled Employment Effects & Power Discrepancies

	Dependent Variable: Victim Employment						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.0746	-0.0679	-0.0653	-0.0501	-0.0736	-0.0560	-0.0500
	(0.0123)	(0.0135)	(0.0135)	(0.0139)	(0.0104)	(0.0111)	(0.0139)
Treatment*Female Victim	-0.0157	-0.0176	-0.0161	-0.0119			-0.0114
	(0.0180)	(0.0180)	(0.0180)	(0.0181)			(0.0181)
Treatment*Perp is Manager			-0.0640		-0.0646		-0.0478
			(0.0272)		(0.0272)		(0.0285)
Treatment*Income Gap				-0.0499		-0.0506	-0.0445
				(0.0140)		(0.0139)	(0.0146)
Treatment*Assault		-0.0153	-0.0101	-0.0126	-0.00904	-0.0117	-0.00903
		(0.0144)	(0.0143)	(0.0144)	(0.0143)	(0.0144)	(0.0143)
Observations	45941	45941	45941	45941	45941	45941	45941
	Dependent Variable: Perpetrator Employment						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.123	-0.107	-0.112	-0.147	-0.0886	-0.128	-0.147
	(0.0124)	(0.0137)	(0.0137)	(0.0149)	(0.0104)	(0.0123)	(0.0149)
Treatment*Female Victim	0.0526	0.0482	0.0457	0.0363			0.0356
	(0.0176)	(0.0177)	(0.0177)	(0.0177)			(0.0176)
Treatment*Perp is Manager			0.105		0.107		0.0701
			(0.0168)		(0.0167)		(0.0170)
Treatment*Income Gap				0.107		0.109	0.0989
				(0.0136)		(0.0135)	(0.0137)
Treatment*Assault		-0.0347	-0.0429	-0.0408	-0.0462	-0.0434	-0.0458
		(0.0145)	(0.0146)	(0.0142)	(0.0145)	(0.0141)	(0.0143)
Observations	46628	46628	46628	46628	46628	46628	46628
Year fixed effects	✓	✓	✓	✓	✓	✓	✓
Time since crime fixed effects	✓	✓	✓	✓	✓	✓	✓
Individual-incident-year FE	✓	✓	✓	✓	✓	✓	✓
Age by time since crime	✓	✓	✓	✓	✓	✓	✓

Notes: Table reports estimates of equation 2, examining the degree to which power differentials explain the difference in employment impacts for perpetrators with male versus female victims. Data is from police reports linked to FLEED administrative register data. Standard errors are clustered at the individual level.

Table VI: Female Management and Impacts on Victim and Perpetrator Employment

	Dependent Variable: Employment			
	Male-Female		Male-Male	
	Victim	Perpetrator	Victim	Perpetrator
	(1)	(2)	(3)	(4)
Treatment*Above Median Female Management	0.005	-0.055	0.000	-0.084
	(0.019)	(0.019)	(0.020)	(0.018)
Treatment	-0.093	-0.043	-0.075	-0.082
	(0.015)	(0.014)	(0.015)	(0.014)
Year fixed effects	✓	✓	✓	✓
Time since crime fixed effects	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓
Observations	22,861	23,253	23,076	23,374
Non-Violent Mean	0.854	0.867	0.857	0.861

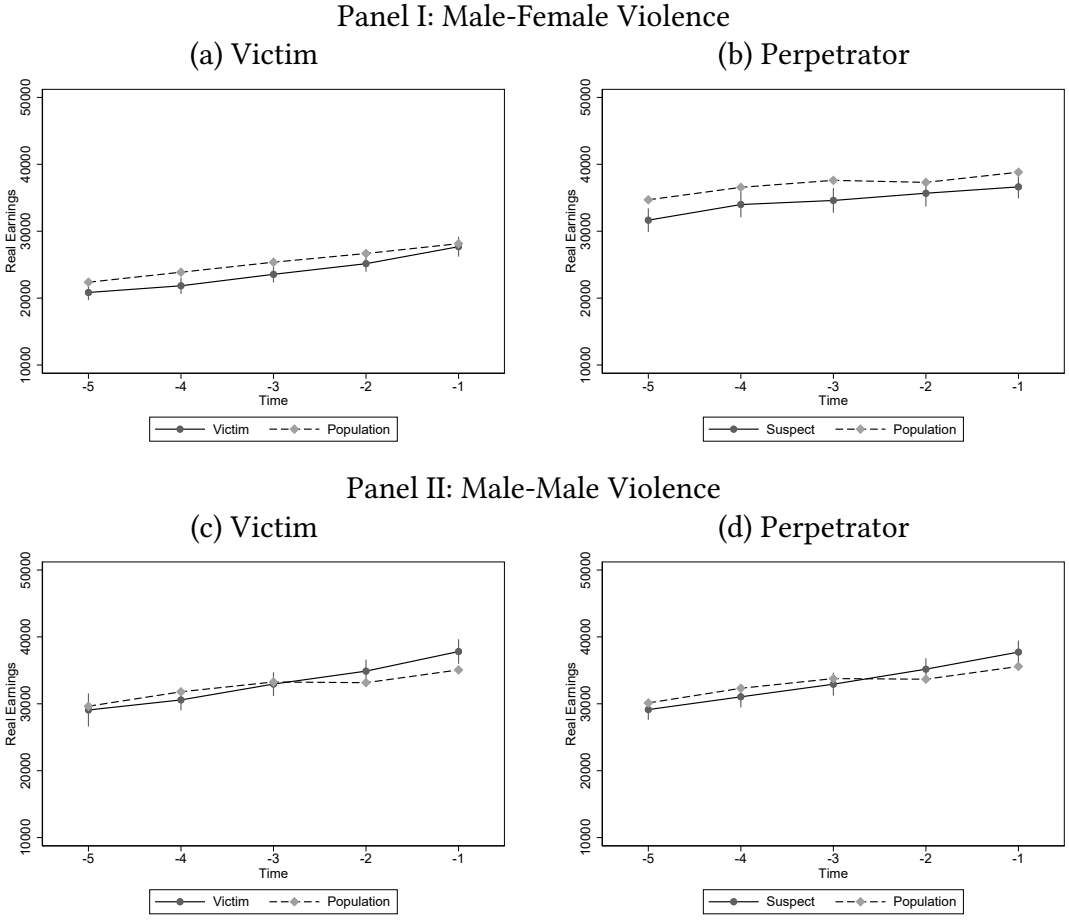
Notes: Table reports the impact of a violent incident between colleagues interacted with whether the firm is female-managed on victim and perpetrator employment outcomes separately for male-female and male-male crimes as specified by column headings. We define management as "male" if the share of men 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. Standard errors are clustered at the individual level.

Table VII: Gender Composition of Management Impact on Share Female Employees

	Dependent Variable: Share Female Employees in Firm					
	Male-Female			Male-Female with Controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Female Management	0.0325		0.0252	0.0307		0.0227
	(0.0090)		(0.0144)	(0.0102)		(0.0149)
Treat*Perpetrator Job Loss		0.0250	0.0247		0.0272	0.0244
		(0.0090)	(0.0155)		(0.0085)	(0.0151)
Treat*Female Management*Perp Job Loss			0.0099			0.0102
			(0.0174)			(0.0172)
Treatment	-0.0227	-0.0387	-0.0386	-0.0136	-0.0393	-0.0353
	(0.0083)	(0.0085)	(0.0135)	(0.0111)	(0.0113)	(0.0152)
Additional controls				✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Time since crime fixed effects	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓
Observations	14,634	14,634	14,634	13,929	13,929	13,929
Dependent Variable Mean	0.4042	0.4042	0.4042	0.2083	0.2083	0.2083

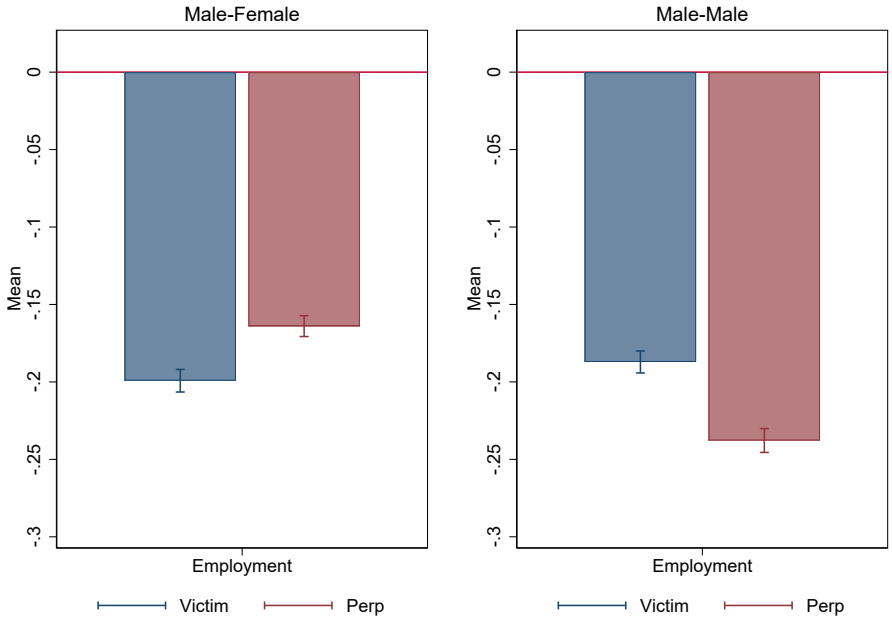
Notes: Table reports the impact of a violent incident between colleagues on the share of women employed in the firm. We define management as "male" if the share of men in the top 20% of earners is above the median share, and "female" otherwise. Perpetrator job loss is a dummy variable equal to one if 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. Standard errors are clustered at the firm level. Additional controls consist of dummies for above or below median firm size, average earnings, average age, average tenure, and share female within the firm, where we interact each separately with the treatment.

Figure I: Earnings Growth of Victims and Perpetrators the Five Years Prior to Violence Compared with Earnings Growth of All Workers in Finland



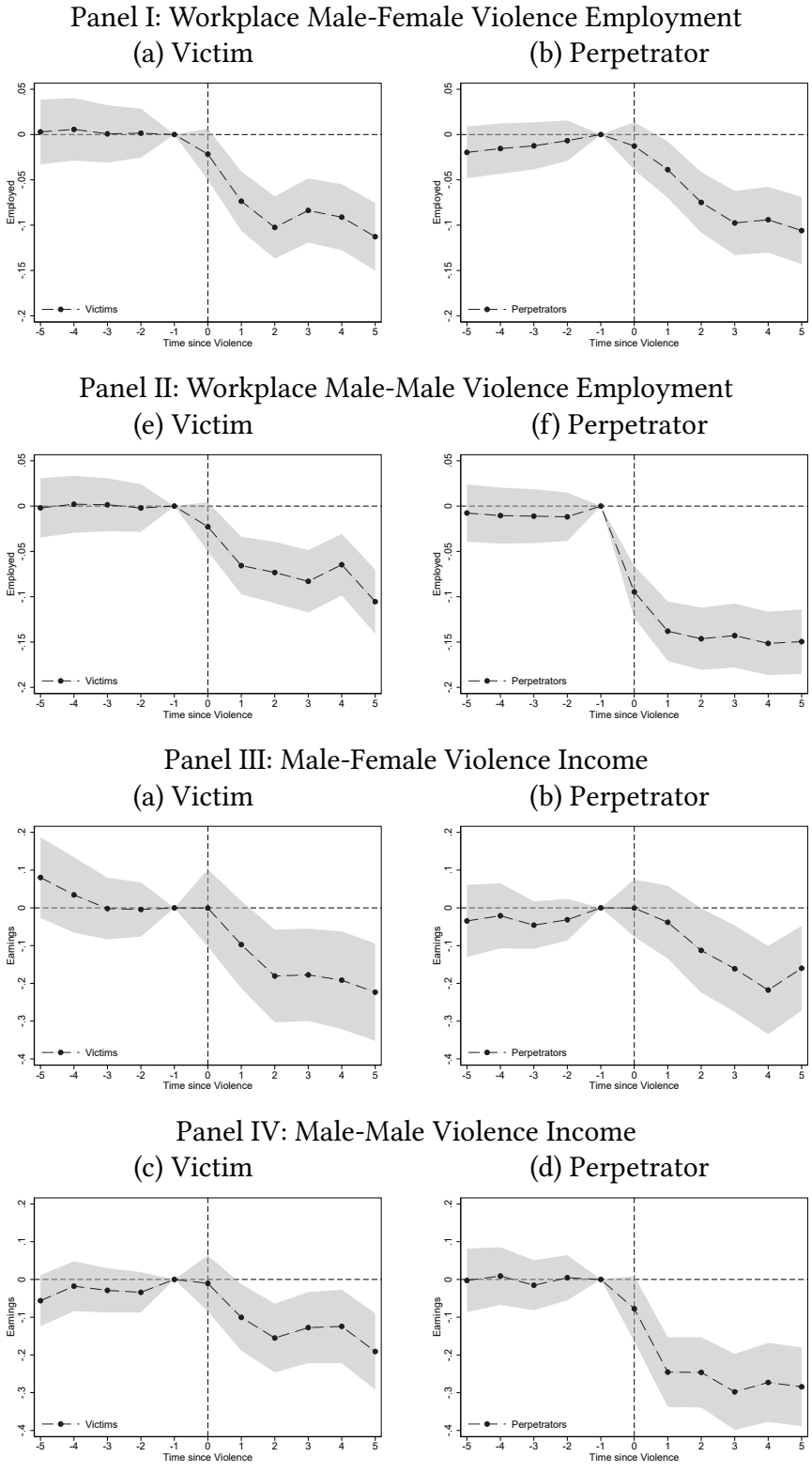
Notes Each figure reports the earnings in the five years prior to the violent incident for the victim (left-hand side) or perpetrator (right-hand side), relative to all workers in Finland of the same gender, and of a similar age and education level. To calculate all Finnish workers' earnings pre-trend, we assign the year 2010 as the pseudo-year of the event for all Finnish workers and calculate their previous 5 years of earnings. First row reports trends for male-female violence. Second row reports trends for male-male violence. Earnings consist of all labor market earnings (the sum of wage, salary, and self-employment earnings) and is deflated to 2013 Euros. The x-axis measures years relative to the year in which violence takes place. We reweight the outcomes of all workers in Finland to match the age and education distribution of victims and perpetrators.

Figure II: Asymmetry in the Raw Impacts of Between Colleague Violence on Employment and Earnings



Notes: Figure reports the change in employment 1 year post-violence compared to the year before violence. Left-hand figure reports this raw employment change 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 for the mean change in employment are depicted in whiskers. The employment measure we use is an indicator for whether an individual was employed during the last week of the year (the reference week).

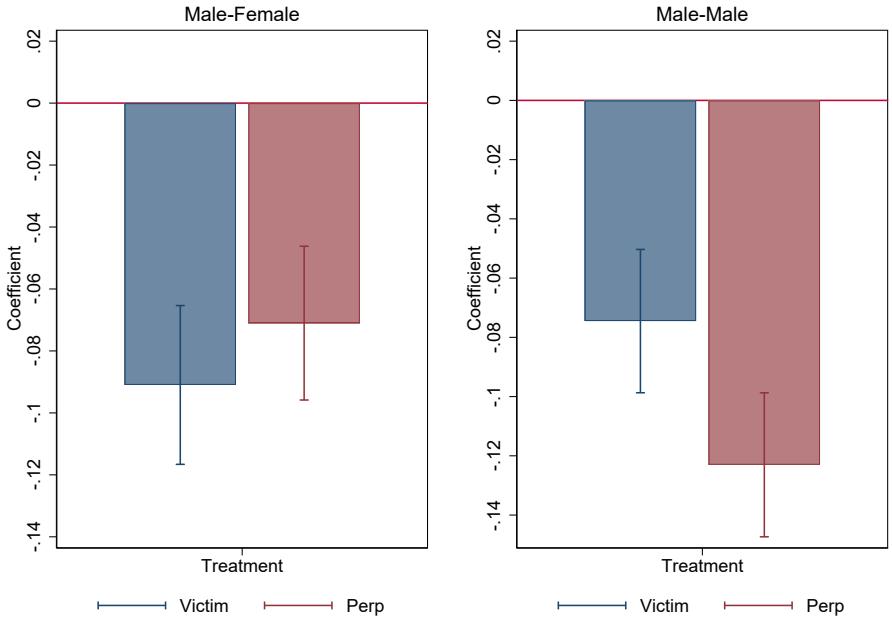
Figure III: Impact of Workplace Violence on Employment and Incomes of Victims and Perpetrators



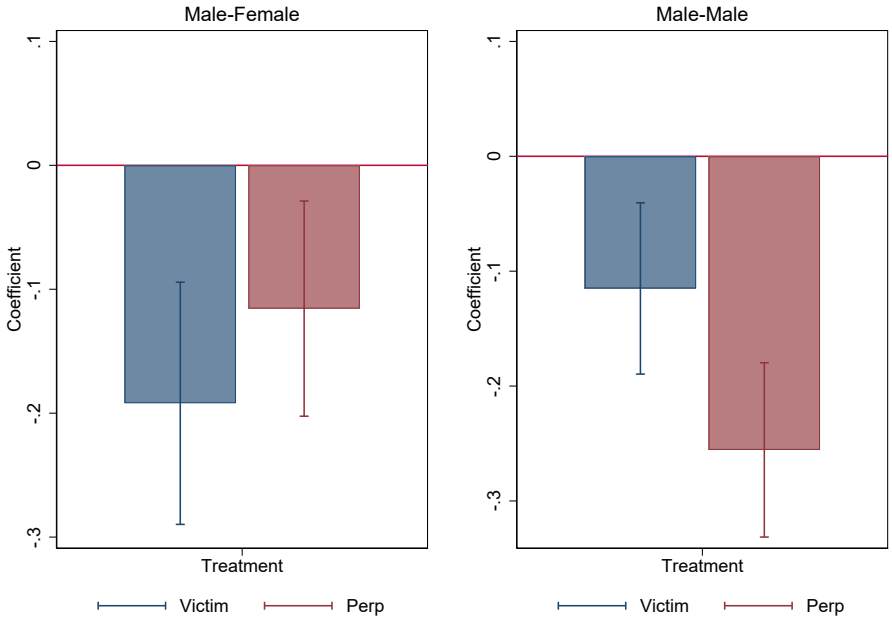
Notes Figures report impacts of between-colleague violence on the victim (left-hand side) or perpetrator (right-hand side). The first (third) row reports impacts of male-female violence on employment (income), while second (fourth) row reports similarly for male-male violence. The estimates use the matched control to identify effects 5 years before and 5 years after a violent incident against a colleague (see equation 1). Employment indicates whether the individual was employed in the last week of the year. Income is total taxable labor market income (the sum of salary, wages, and self-employment earnings), and we estimate impacts measured as all taxable income at the end of the year as a fraction of the total income in the year before the incident. Standard errors are clustered at the individual level.

Figure IV: Asymmetry in Employment and Earnings Impacts of Workplace Violence

(a) Employment

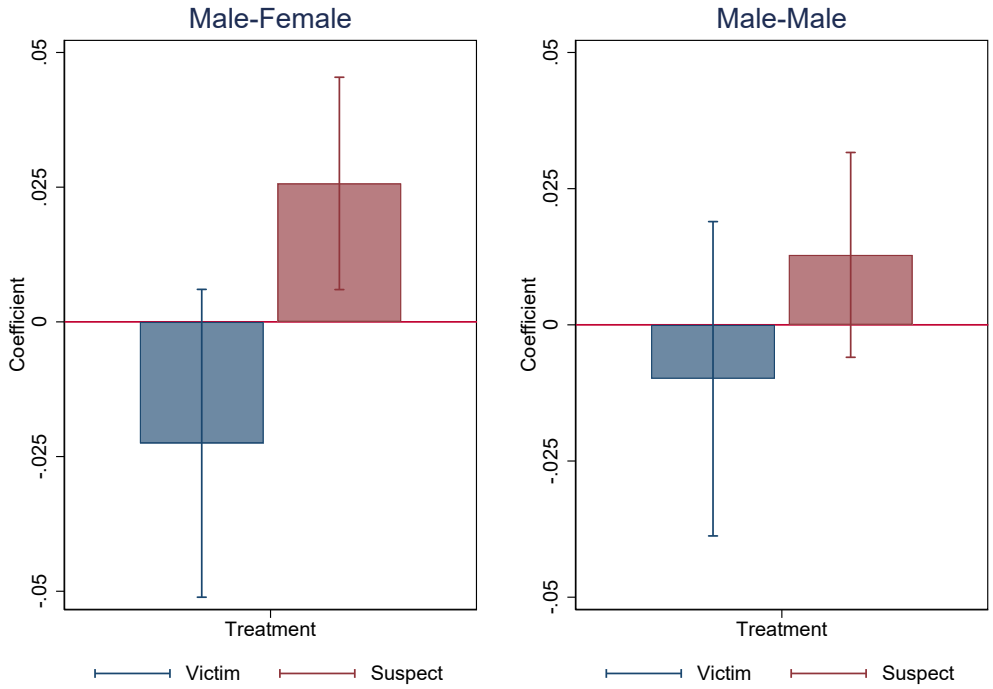


(b) Labor Market Income



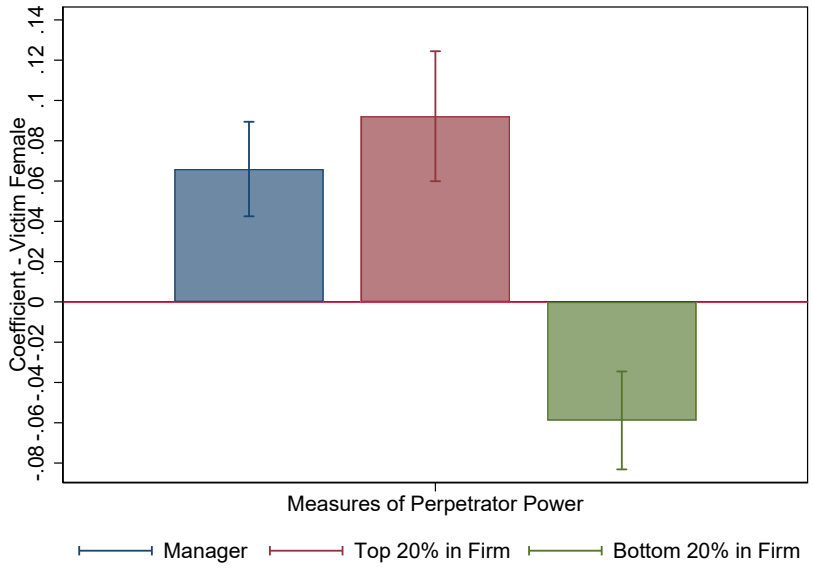
Notes: Figure reports estimates of δ_t obtained using Equation (1) where we collapse into a pre- and post-period to recover difference-in-differences estimates. Left-hand figure reports DiD estimates for male-female violence for victims (in the blue bar on the left) and perpetrators (in the red bar on the right). Similarly for male-male violence in the right-hand figure. Panel A reports impacts on employment while Panel B reports impacts on labor market income. 95% confidence intervals are depicted in whiskers around the estimates. Employment indicates whether the individual was employed in the last week of the year. Income is total taxable labor market income (the sum of salary, wages, and self-employment earnings) after the event as a fraction of the average total income in the 5 years prior. Standard errors are clustered at the individual level.

Figure V: Comparing Workplace and Non-Workplace Violence Impacts on Employment



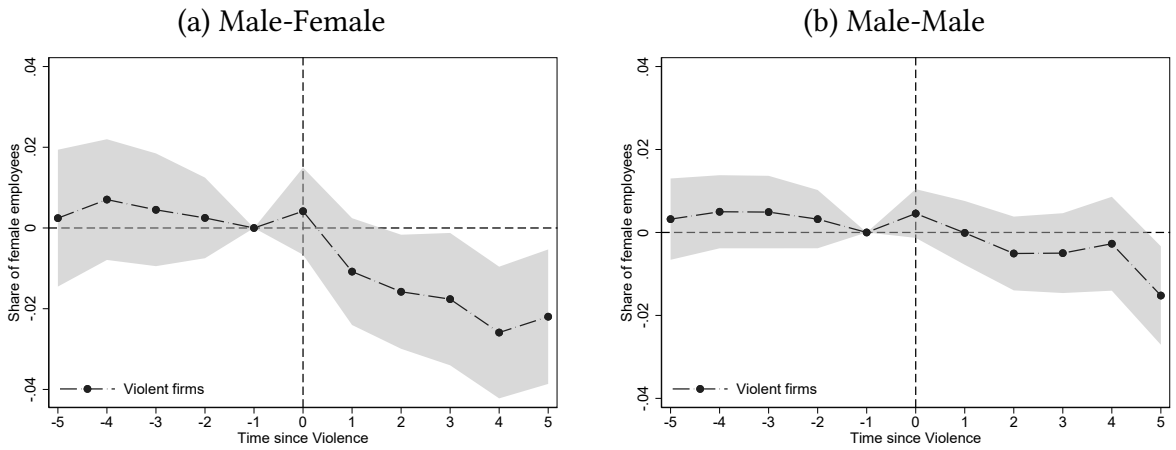
Notes: Figure reports estimates of δ_t obtained using Equation (1) where we collapse into a pre- and post-period to recover difference-in-differences estimates. However, unlike the main estimates which use a nearest neighbor match who is not also a victim (or perpetrator when estimating perpetrator impacts) 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 I, but where the two did not work in the same plant, and so were not colleagues. 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 are depicted in whiskers around the estimates. Employment is measured at the end of the year. Standard errors are clustered at the individual level.

Figure VI: Perpetrators Who Attack Women Are More Powerful Within the Firm



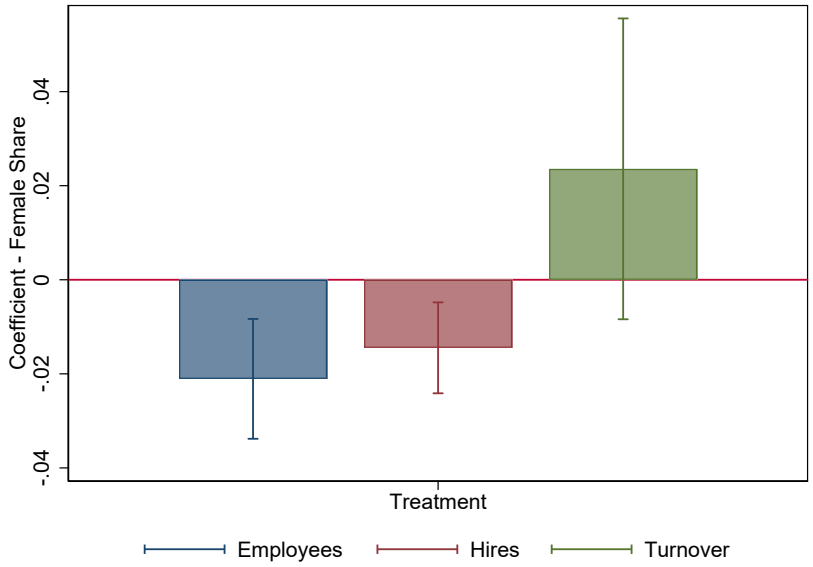
Notes: Figure reports descriptive estimates of the correlation between victim gender and the power of the perpetrator, focusing on whether the perpetrator is a manager (in the blue, leftmost bar), whether the perpetrator is in the top 20% or earners in the firm (in the red, middle bar), and whether the perpetrator is in the bottom 20% of earners in the firm (in the green, rightmost bar). 95% confidence intervals depicted in whiskers.

Figure VII: Impact on Share Female Employees in the Firm



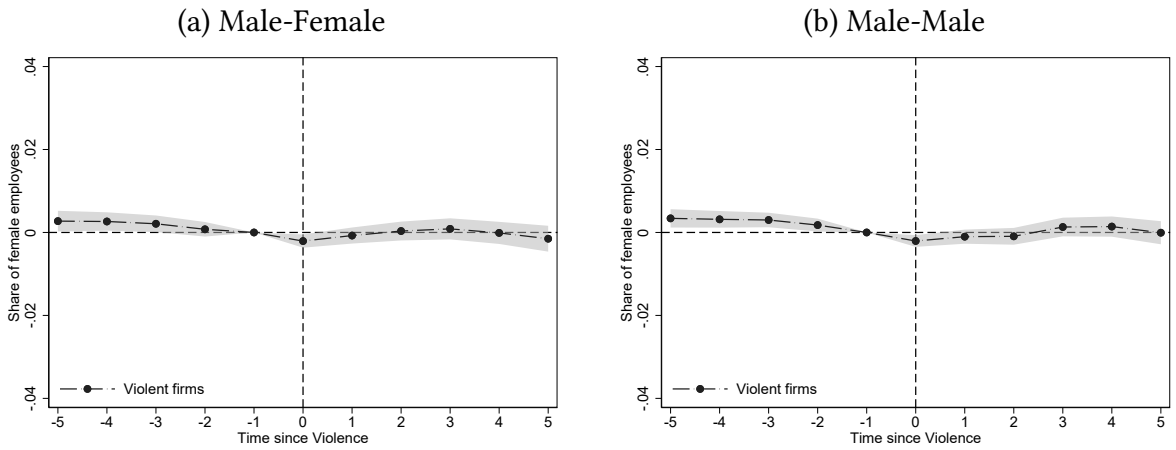
Notes: Left-hand-side figure shows the impact of a violent incident between colleagues on the share of female workers at the firm in which both perpetrator and victim were employed at the time of the incident for male-female between-colleague violence, the right-hand-side figure shows the same impact but for male-male between-colleague violence. The estimates use the matched control DiD design 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. The horizontal axis displays time in years. Dashed vertical lines indicate the year of between-colleague violence. Standard errors are clustered at the firm level.

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



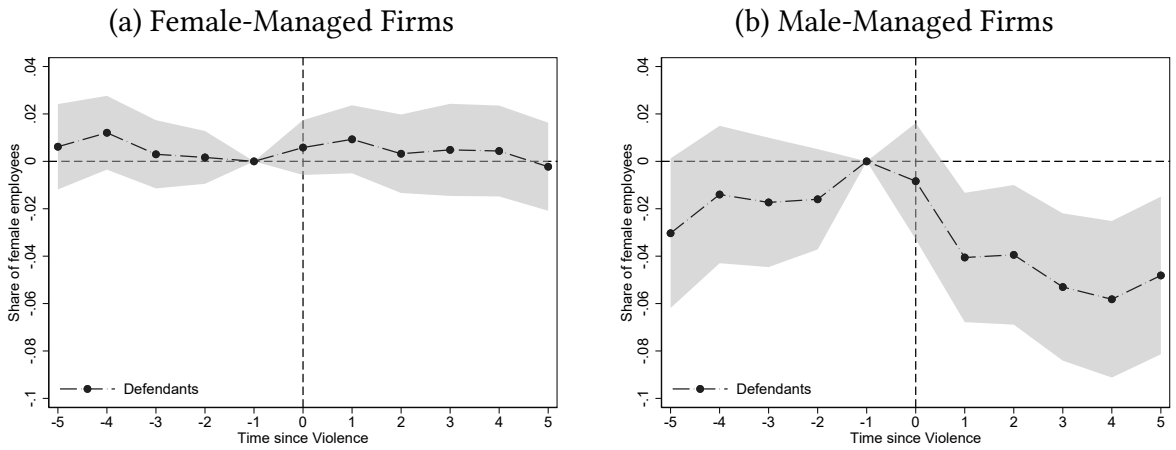
Notes: Figure reports DiD estimates of the impact of between colleague violence on the overall share of women in the firm (in blue, leftmost bar), the share of women amongst new hires (in red, middle bar), and female turnover in the firm (in green, rightmost bar). Impacts shown for male-female between-colleague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

Figure IX: Impact of Non-Colleague Violence on the Share Female Employees in the Firm



Notes: Left-hand-side figure shows the impact of a violent incident 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 where the perpetrator is employed for male-female between-colleague violence. The right-hand-side figure shows the same impact but for male-male between-colleague violence. The estimates use the matched control DiD design to identify effects 5 years before and 5 years after a violent incident against a colleague. The horizontal axis displays time in years. Dashed vertical lines indicate the year of between-colleague violence. Standard errors are clustered at the firm level.

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



Notes: Figure shows the impact of male-female violence on the share of female workers in the firm separately for female-managed firms (left figure) versus male-managed firms (right 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. Standard errors are clustered at the firm level.