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VIOLENCE AGAINST WOMEN AT WORK*

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Abstract

The #MeToo movement has demonstrated that assaults between colleagues are an internationally relevant phenomenon. In this paper, we link every police report in Finland to administrative data to identify assaults 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 women at the firm, both because fewer new women are hired and current female employees leave. There is no change in hiring from within existing employees' networks, ruling out supply-side explanations for the reduction in new female hires via "whisper networks". Management practices play a key role in mediating the impacts on the wider workforce. Only male-managed firms lose women. Female managers do one important thing differently: fire perpetrators.

JEL Codes: J16, J81, M54

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1 Introduction

The #MeToo movement demonstrated that violence between colleagues is an internationally relevant phenomenon. Many accounts were characterized by high-profile men in positions of power assaulting female subordinates with few repercussions. Yet there is little empirical research on the impacts of workplace related violence on perpetrators, victims, and the wider workforce. 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 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. Is it only the rich and powerful who go unpunished, or do unremarkable managers in nondescript offices also enjoy less severe consequences if they assault a subordinate?

In this paper, we harness unique Finnish administrative data to analyze the impact of 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.²

This new methodological approach to observe when one colleague attacks another and what happens afterwards 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 might affect 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

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

²We discuss the implications of under-reporting for the interpretation of our estimates in Sections 2 and 6.

on this topic from economics, psychology, and sociology to self-reports from surveys, which do not allow perpetrators to be identified.³

We identify over 5,000 cases of violence between colleagues. If we take survey evidence that only 10% of assaults in Finland are reported to police seriously, then 4% of individuals in the labor force are directly involved in violence with colleagues. Even more are impacted through spillovers we will document on the broader workforce. 57% of incidents between employees are assaults or petty assaults, and the remaining 43% are a mix of negligent bodily injury, menace, and other crimes. The vast majority (83%) of perpetrators are men, while victims are evenly split between male and female. Victims of male-female violence are relatively low-earning women within the firm compared to their perpetrators who are relatively high-earning men. This is not the case for male-male workplace crimes, where 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 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 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).⁴

The precipitous employment losses following violence between colleagues in the raw data

³See discussion in Section 2 for more details.

⁴We do not separately analyze female perpetrated workplace violence since a) women rarely attack colleagues resulting in small sample sizes and b) in the majority of cases where a woman is recorded as the perpetrator, she is also recorded as a victim, thus these are not clear-cut cases of female perpetrated violence. See Section 2.2 for details.

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

We 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 5.6 (7.4) percentage points *more* when their perpetrator is a manager. However, perpetrators who are managers are 5.9 (13.3) percentage points *less* likely to be unemployed in the five years following an incident. Results are similar when using the difference in the within-firm income rank of perpetrators and victims as the measure of relative power. Thus, perpetrator power 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.

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.⁵ 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. The reduction in the share of employees who are women in firms where male-female violence takes place is explained both by higher separation rates of existing female employees and a significant reduction in the share of women amongst new hires.

While the rise in female exits following male-female violence is relatively straightforward to interpret, falls in the proportion of women hired by the firm could be driven by supply-side factors, i.e. women being less likely to apply for jobs at the firm, or by demand-side factors, i.e. women being less likely to be hired from a given set of applicants. To investigate whether a supply-side explanation is operative, we analyze changes in hiring rates from existing employee networks. A large literature demonstrates that new hires are often recruited from within existing employee networks (Bayer *et al.*, 2008; Burks *et al.*, 2015; Beaman and Magruder, 2012; Brown *et al.*, 2016; Dustmann *et al.*, 2016), with over 30% of employees finding jobs through personal networks (Barwick *et al.*, 2019). These informal networks are the most likely way individuals learn about colleague violence, given that perpetrator and victim names in police reports are not public. This type of supply-side mechanism would be consistent with the idea of "Whisper Networks", a term popularized during #MeToo to reflect the informal dissemination of information amongst women regarding bad men and bad firms to avoid.

We construct the network of past colleagues from the previous ten years for every employee at the violent and matched control firms (Hensvik and Skans, 2016). We find zero impact on within network hiring. These results suggest that informal approaches of information sharing, often touted as a means by which women might avoid abuse and harassment at work, may not be very effective. We conclude that the drop in the share of female new hires is more likely explained

⁵This is true even when excluding victims and perpetrators from the analysis.

by demand-side factors, i.e. firms choose to hire fewer women following male-female violence.

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

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

We employ a number of exercises to eliminate other possible explanations for our results. A placebo test estimating impacts prior to the event when no violence takes place shows that employment of perpetrators/victims and their matched controls evolve identically before and after the placebo "event" of no violence. This provides reassurance for the main assumption that employment evolves along common trends absent violence. A placebo check for the firm outcomes similarly confirms our main results. Moreover, our results are robust to fuzzier matched difference-in-differences, using future victims/perpetrators as the difference-in-differences counterfactual, and a number of other checks. Together, these results suggest that the only remaining confounder is a different unobserved shock that occurs at the exact same time as the violent in-

cident. We consider it implausible that our results are explained by an orthogonal shock to the obvious one, i.e. that one colleague attacks another, which can jointly explain our four main outcomes: 1) the impacts on the perpetrator; 2) the impacts on the victim; 3) the asymmetry in impacts between male-male and male-female violence; and 4) the broader impacts on the firm. However, we discuss this possibility in more detail in the text.

Our findings contribute to two broad literatures. Most closely related is a small but growing literature showing that women disproportionately experience costly interactions with 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 workplace 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, a relative lack of consequences for their male perpetrators, and this can be explained 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 practices can play a role in mitigating these broader costs. These broader repercussions for peers within the firm complements a large literature documenting spillovers amongst peers in the workplace (Brune *et al.*, 2020; Papay *et al.*, 2020; Nix, 2020; Cornelissen *et al.*, 2017; Waldinger, 2012; Mas and Moretti, 2009; Thornton and Thompson, 2001), as well as in other contexts (Black *et al.*, 2013; Bayer *et al.*, 2009; Hoxby, 2000).

Second, our paper makes an important contribution to the literature on firms and firm management on worker outcomes (Bertrand and Schoar, 2003; Bloom *et al.*, 2007; Bandiera *et al.*, 2007; Ichniowski *et al.*, 1995; Alan *et al.*, 2021). We show that extreme and toxic events in workplaces result in large costs for direct victims, especially female victims, cause other women in the firm to leave, and change the hiring patterns of firms. The gendered aspects of the firm managerial response to these events is most closely related to Sarsons (2017), Chakraborty *et al.* (2021), and Egan *et al.* (2022) described previously along with Benson *et al.* (2021) who show that male managers are less likely to promote female subordinates who are equally productive by scoring them lower in terms of potential and Cullen and Perez-Truglia (2019) who find that male managers are more likely to promote male subordinates.⁶

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

2 Data and Descriptive Statistics

2.1 Data and Defining Workplace Violence

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

⁶Also related is Stoddard *et al.* (2020) who shows that women who find themselves in the minority within a group in a professional setting are at a disadvantage and viewed less favorably by their peers.

Importantly, the police data contains individual identifiers for both the perpetrator and victim, and the employer-employee register data contains both individual identifiers and a unique identifier for the plant and firm at which a worker is employed. This allows us to identify police reports between colleagues employed at the same establishment, a key innovation of this paper.⁷ Hereafter, we will refer to the "establishment/plant" as the "firm", as we will not focus on the broader firm identifiers (for example, we will focus on each individual McDonald's plant as the "firm" of interest as opposed to McDonald's as a whole).

We classify an incident recorded in a police report as workplace violence if both the victim and perpetrator worked at the same plant in the December just preceding or the December just after an incident (we only observe place of employment in December of each year). We include colleagues in the December after an incident as some victims (or perpetrators) may have joined the firm in the preceding 11 months and be involved in between-colleague violence, although we show our results are slightly larger in magnitude and the asymmetry in impacts remains when omitting this group who make up a small minority of all cases (see Appendix Figure B.16).

After merging the police data with the rich administrative data on individual labor market outcomes, we construct the labor market trajectories of victims and perpetrators before and after an incident. For victims and perpetrators, we focus primarily on employment as measured in December of each year. In the Appendix we also report impacts on income, which consists of all labor income reported to the tax authorities and measured in December of each year.⁸ Moreover, we use the plant identifiers from the full population register data to construct relevant firm outcomes such as headcount, turnover, and the gender composition of the workforce. The plant identifiers also allow us to construct labor market outcomes of other workers employed at the same firm which we use in later analysis.

The limited previous literature on workplace violence in other disciplines, such as sociology and psychology, almost solely relies on survey data and small selected samples. For example, an applied psychology literature suggests that violence and harassment can be a key workplace en-

⁷Person, workplace, and firm identifiers are unique ensuring matches are perfect.

⁸We focus on labor income, as we are interested in labor market impacts. We do not include all taxable income. However, it is possible to additionally examine alternative income streams reported to tax authorities, such as certain benefits and capital income.

vironment factor with implications for individual and organizational performance (Cortina *et al.*, 2001; Lim *et al.*, 2008; Estes and Wang, 2008; Geck *et al.*, 2017). Thus, the use of administrative data to study this question is new. This type of data not only allows us to examine direct impacts on victims, but makes it possible to examine outcomes that have never been studied in this context before, such as impacts on perpetrators (prior papers have focused almost exclusively on victims), the role of the within firm economic relationship between victim and perpetrator, impacts on the broader firm including workplace colleagues and new recruits, and the role of management.

Measurement Discussion Our measure of workplace violence captures the population of violent incidents that occur between two colleagues and are reported to the police. These incidents do not necessarily occur in the workplace itself: the police data does not include the precise location of the crime. This means that the incidents we study could be happening both inside and outside the official premises of the firm. 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. Many anecdotes from the #MeToo movement described events between colleagues that occurred outside of working hours and away from the physical office. Using our definition of workplace 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. While technically these are also events of between colleague violence, it is not clear if we would want to include them in the analysis. However, we do not view this as a major concern as we find that fewer than 2% of domestic violence incidents occur between colleagues. Nonetheless, in Section 3.5 we show that our results are robust to excluding 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 (EU Agency for Fundamental Rights, 2015; Eu-

ropean Institute for Crime Prevention & Control, 2009). Our measure therefore understates the true prevalence of workplace violence and likely captures the most serious incidents. However, survey evidence suggests that reporting rates for non-partner assault are similar for male and female victims (EU Agency for Fundamental Rights, 2015; European Institute for Crime Prevention & Control, 2009). This evidence suggests that the coverage of our measure should not differ significantly by gender. If we take this 10% undercount at face value, then 4% 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 all those impacted by these events, given spillovers on the broader workforce within the firm. We 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 1 provides an overview of the types of crimes between colleagues in the police report data. In Table 2, we report summary statistics for the characteristics of victims and perpetrators of between-colleague violence in the year before an incident. Table 3 gives the characteristics of firms in which violence takes place and compares their characteristics to the rest of firms in Finland.¹⁰ Between 2006-2019, there were over 5,200 police reports of violence between colleagues in Finland. Table 1 shows that the majority of these incidents were one of four crime types: assault (36.8%); petty assault (20.3%); menace (13.8%); and negligent bodily injury (13.5%).

⁹According to Table 2, 10,151 individuals are involved in between colleague violence. If the true number of assaults is 10 times larger, this translates to 101,510 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 $101510/2684521=3.8\%$ of those in the labor force during this time were directly involved with colleague violence.

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

Table 1: Crime Types

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

Notes: The table reports the types of between-colleague crimes for the full sample, the male-female violence sample, and the male-male violence sample. In Table 2 we collapse the data to the individual level within year, which is the level at which we observe individual characteristics, i.e. if an individual commits multiple workplace crimes in the year he is only included once. Thus, Table 2 has fewer observations than this table, since in this table multiple crimes in the same year are all included as separate observations.

Individual Characteristics Table 2 shows that approximately half of victims are women. In contrast, 83% of perpetrators are men. Almost by construction, victims and perpetrators of colleague violence have significantly greater labor market attachment than those involved in non-colleague violence: over 91% are working the year before the incident.¹¹ Table 2 reveals some key differences in the characteristics of workplace violence involving a male perpetrator and female victim (hereafter male-female violence) compared to those involving a male perpetrator and male victim (hereafter male-male violence) that go beyond simply victim gender. On average, victims and perpetrators of male-male workplace violence are the same age and have similar incomes. Male-male crimes are also slightly more likely to have multiple perpetrators compared with male-female crimes, where almost all crimes consist of a single perpetrator.

In contrast, victims of male-female workplace violence are younger and earn €11,807 less than their male perpetrators, equivalent to 32% lower incomes. On average, the income gap between female victims and male perpetrators is approximately twice as large as the average gender pay gap within the firm (see Table 3). Additionally, male perpetrators of male-female workplace

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

Table 2: Sample Means for Perpetrators and Victims

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

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

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.

The types of crimes that characterize male-female workplace violence are also more serious than those for male-male workplace violence. Table 1 shows that 66% of male-female incidents are assaults compared to 48% of male-male incidents. Beyond assault, the next most prevalent crime for male-male workplace violence is "negligent bodily injury", which is characterised by a lack of care, while for male-female violence it is "menace", which requires a perpetrator intentionally causing fear of serious injury or death.¹² Note that Table 1 reports statistics at the case level, and thus has more observations than Table 2 which collapses the data to the individual level within year, i.e. if an individual commits multiple workplace crimes in the year, they are only included once in Table 2.

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

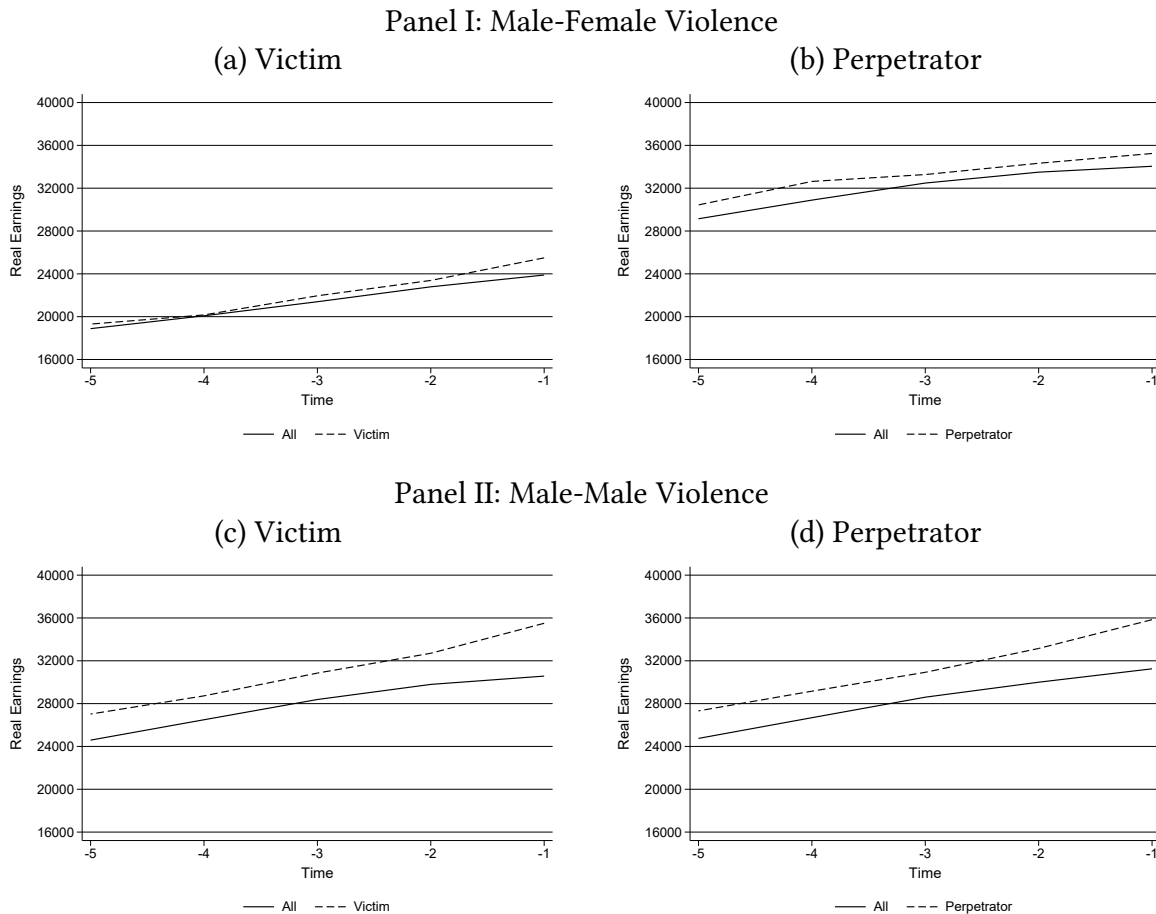
¹²The greater severity of male-female crimes is also apparent if we look at the rate at which these types of crimes are incarcerated. A much higher share of menace and assault cases in court result in prison sentences (9.5% and 7.3% respectively), compared to negligent bodily injury (0.6%). Note that these rates are for all such cases, and do not restrict to cases of between-colleague violence.

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 deterioration in earnings for perpetrators prior to the reported event. This is not what we find. In Figure 1 we graph earnings of victims and perpetrators compared to all workers in Finland 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 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 1 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.

¹³Appendix Figure B.3 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 either the year before the event (-1) or year of the event (0) for it to be violence between colleagues), the earnings growth is not.

Figure 1: 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 a similar age and education level. First row reports effects for male-female violence. Second row reports effects for male-male violence. Earnings is total earnings measured in December of each year, and 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.

Firm Characteristics Turning to firm characteristics, Table 3 shows that firms in which colleague violence is reported do not appear to be negatively selected relative to all other firms in Finland. They have higher wages and higher tenure. This table also shows that firms where male-female crimes take place are not obviously negatively selected for female employees. They have a higher female share overall, a higher share of female new hires, and smaller gender pay gaps compared to male-male violent firms, and approximately the same female share and gender pay gaps as all other firms. However, it is worth noting that male-female violence occurring in

firms with a reasonable share of women may be partly mechanical. A firm must have at least one woman for male-female violence to take place. The dimension along which violent and non-violent firms differ the most is firm size, as violent firms are significantly larger. This is partly mechanical: in larger firms, there are more potential combinations of colleagues between whom violence can occur.

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

Table 3: Firm Summary Statistics

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

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

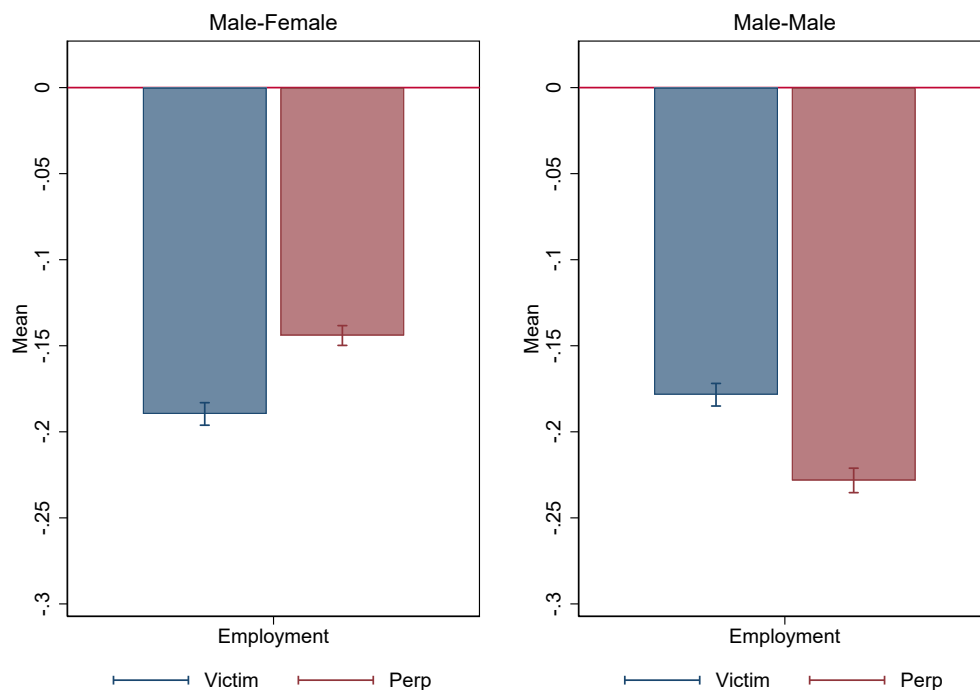
3 Impacts on Victims and Perpetrators

3.1 Descriptive Labor Market Outcomes

Figure 2 gives the raw change in employment rates of perpetrators and victims the year after relative to the year before an incident. 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 almost a 20 percentage point decline in their employment while their male perpetrators experience a smaller decline in employment of just under 15 percentage points the year after the event compared to the year before. Male perpetrators of male-male violence experience a 23 percentage point decline in employment compared with an 18 percentage 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 1, and Appendix Figures B.3-B.5).

Figure 2: Asymmetry in the Raw Impacts of Between Colleague Violence on Employment



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. Standard deviations depicted in whiskers around the estimates. Employment is measured at the end of the year.

3.2 Estimated Labor Market Outcomes

The raw means depicted in Figure 2 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 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 (Aneja and Xu, 2022; Goldschmidt and Schmieder, 2017). This approach allows us to carefully compare the evolution of outcomes before and after the incident for treatment and control observations with similar characteristics. 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 neighbour match on the basis of their age, education level, gender, employment and income history in the five years before the incident.¹⁴ We restrict to victims and perpetrators that we can follow 5 years before until 5 years after the event. For firm outcomes, we will find the nearest neighbor match on firm characteristics: firm size, turnover rate, industry, average age of workers, average education of workers, share of new hires, and gender composition.¹⁵ These exercises leave us with a match for the victim, a match for the perpetrator, and a match for the firm that all appear identical on observables in the five years prior to the year in which violence occurs, but who do not experience workplace violence.¹⁶

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

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

¹⁴In the matching, we control for age and log income linearly; and control for education level, gender, and employment by fixed effects. For perpetrators, we also control for the number of cumulative crime records linearly.

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

¹⁶Note we don't restrict to same-firm matches as they are also treated given colleague spillovers we will document.

Y_{ibt} represents the outcome of interest for victim (perpetrator) i in base-year sample b at time t .¹⁷ 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 event fixed effects, γ_j , and age at the time of incident by time since event interactions, $Age_{ib} * \gamma_j$.¹⁸ Standard errors are clustered at the individual level (or firm level when examining firm outcomes). Our results are robust to adding incidence-year specific time-since-event fixed effects.

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

A key assumption for a causal interpretation of the 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 B.4 and B.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

¹⁷We primarily focus on employment outcomes, although in the Appendix we also examine impacts on income. 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 .

¹⁸People are different ages at base year, so this is not collinear with individual and time since event fixed effects.

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 3.5 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 3.5, after presenting the main estimates.

Results Figure 3 gives the event study coefficients of interest from Equation 1 for victims and perpetrators with employment as the outcome of interest. Figure 4 reports the aggregated difference-in-differences results.¹⁹ In the first row of Figure 3 we see that colleague violence leads to immediate, large, and persistent drops in employment for both victims and perpetrators.²⁰ In the second row, we see that the employment rate of victims of male-female violence falls 4.6 percentage points in the first year after the incident relative to their matched controls, which grows to a 10 percentage point fall in employment by five years later. Perpetrators of male-female workplace violence also experience negative impacts to their employment.²¹ The last row of Figure 3 reports event-study coefficients for victims and perpetrators of male-male violence. Victims see an immediate 2.3 percentage point drop in employment that grows to 7.1 percentage points 5 years later. Perpetrators see an immediate employment decline of 10 percentage points which grows to 11.5 percentage points five years later.

The event studies not only show that workplace violence has large economic consequences for victims and perpetrators. They also demonstrate a striking asymmetry in the labor market impacts across male-female and male-male violence, consistent with the raw descriptive results. Figure 4 succinctly summarizes this, showing the overall DiD impacts on the perpetrators and victims of male-female versus male-male crimes. For male-male violence, perpetrators suffer a

¹⁹Table A.4 gives the aggregated difference-in-differences results and results from various robustness exercises.

²⁰Note that the slight increase in employment at time zero is mechanical, due to our requiring employment at the time of the incident in either year -1 or year 0 and the fact that we do not match the counterfactual control in year zero (see Section 2 for details). Appendix Figure B.6 gives the equivalent results for percent income losses.

²¹In the five years after the incident, there is a 5.2 percentage point drop in perpetrator's employment and an 8% fall in income relative to the pre-violence baseline (see Table A.4).

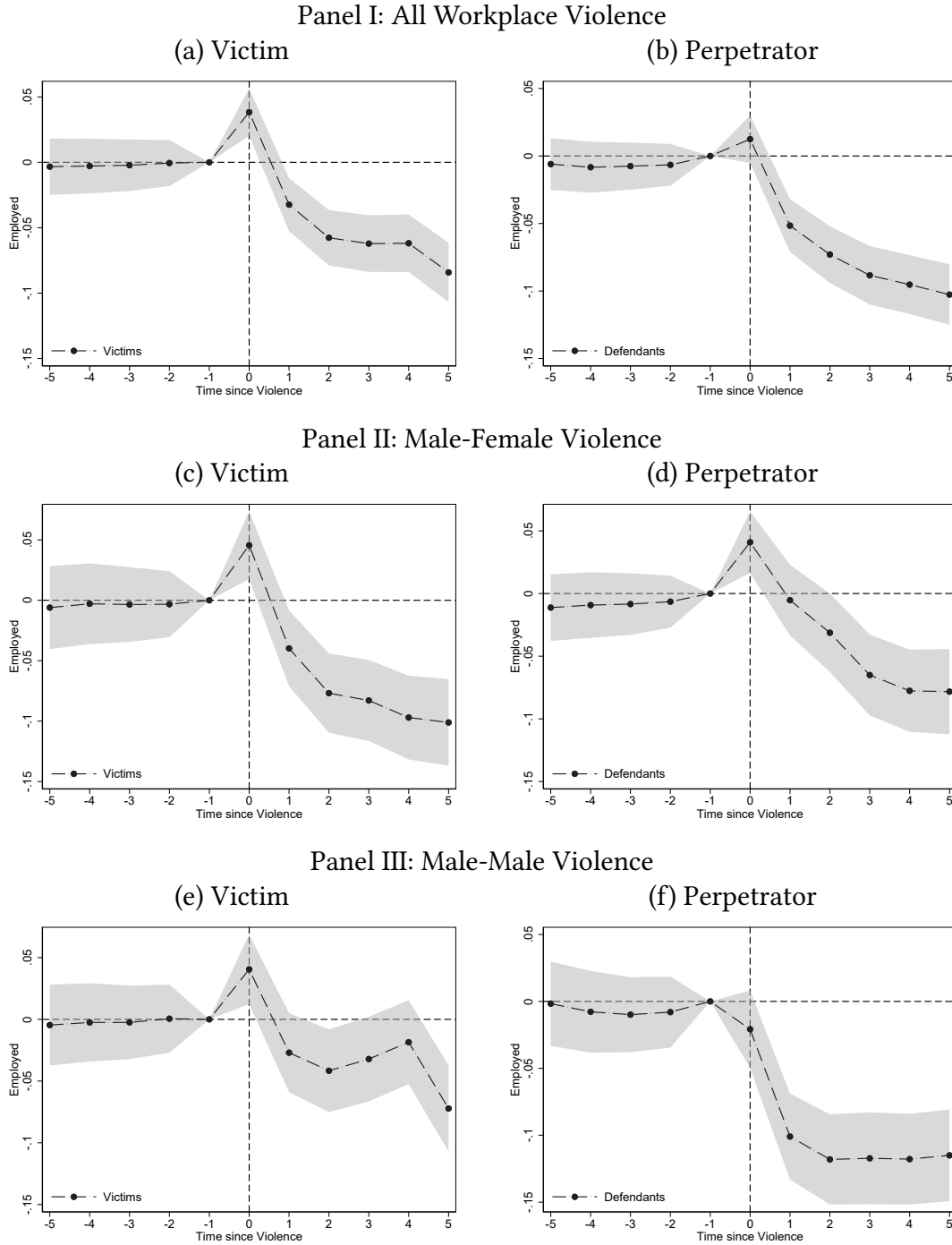
significantly greater labor market cost than their victims: their employment rates fall by over 10 percentage points in the five years following an incident, and this impact is significantly different than the 4.2 percentage point decline in employment for victims. This is the result one might expect. It seems reasonable and appropriate that perpetrators of workplace violence experience more negative outcomes after attacking a colleague than their victims. In particular, we might expect the firm to punish perpetrators after the incident, including possibly firing the perpetrator.

In contrast, there is no statistically significant difference in the employment impacts of male-female violence across perpetrators and victims. Indeed, if anything, perpetrators experience smaller labor market consequences compared to their victims. Overall, perpetrators' employment rates fall by 5.2 percentage points. This is smaller than, although not statistically distinguishable from, their female victims whose employment falls 8.4 percentage points. We see a similar asymmetry in the impact of workplace violence on the percent earnings losses of victims versus perpetrators for male-female versus male-male crimes, see Appendix Figure B.7 and column (3) of Appendix Table A.5.²²

In Appendix Figure B.8 we examine the impact of a violent incident on leaving the firm. 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 the same asymmetry in the perpetrator impacts of workplace violence. First, there is no difference between perpetrators of male-female violence and their matched control in whether they remain in the same firm. Combined with Figure 4, this is consistent with perpetrators who are not fired being *more* likely to remain at the same firm relative to their matched counterfactual. Thus, conditional on not being let go and moving into unemployment, perpetrators of violence against female colleagues are more likely to remain in the firm following the incident. In contrast, male-male perpetrators are 8 percentage points less likely to be employed in the same firm. 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.

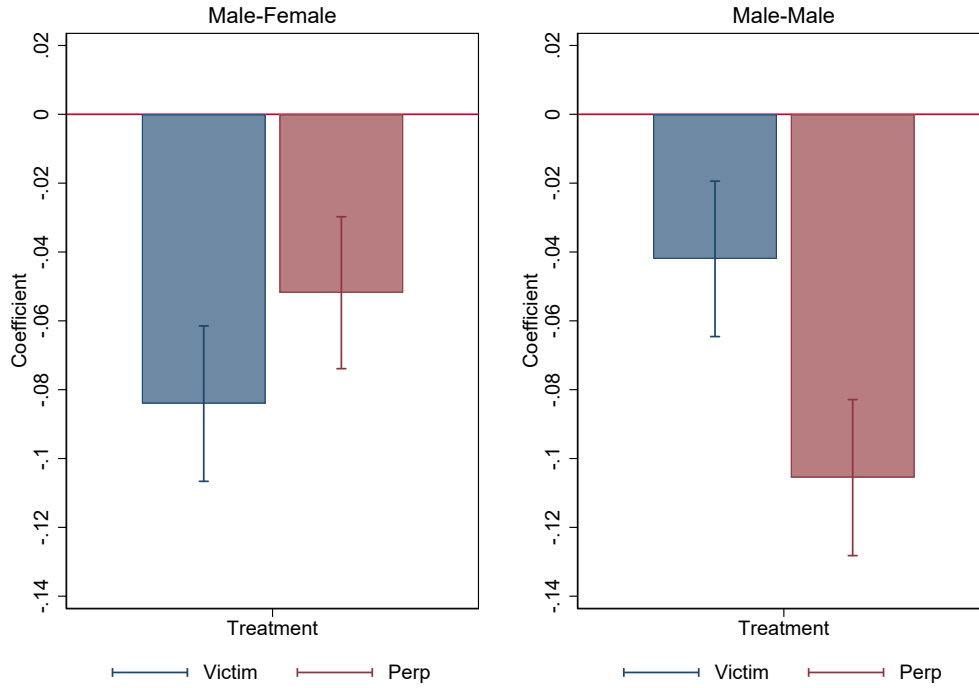
²²Victims of male-female crimes experience a loss in income of €2,198 on average (see Appendix Table A.5). This is a sizable magnitude: column (3) of Table A.5 shows that female victims' incomes fall 16% on average compared to the pre-violence baseline.

Figure 3: Impact of Workplace Violence on Employment of Victims and Perpetrators



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

Figure 4: Asymmetry in Employment Impacts of Workplace Violence



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. 95% confidence intervals depicted in whiskers around the estimates. Employment is measured at the end of the year. Standard errors are clustered at the individual level.

3.3 The Role of Power Discrepancies Between Victim and Perpetrator

Figures 2 and 4 document asymmetry in outcomes for perpetrators and victims of male-male versus male-female colleague violence, without exploring underlying mechanisms. In this section we explore one possible explanation for this asymmetry: the economic standing of perpetrators relative to victims within the firm. Anecdotal evidence from the #MeToo movement suggests that inequality between victims and perpetrators, and being attacked by an individual in a position of power, is especially problematic for victims. When describing the aftermath of her assault at the hands of Harvey Weinstein, Rowena Chiu wrote *"Harvey was a power player, and I was the lowest person on the totem pole. Assistants are the unseen work force that props Hollywood up, and*

yet we have zero leverage. I was invisible and inconsequential." (Chiu, 2019).²³ This was a common theme for #MeToo victims, even when the perpetrators were not as famous and powerful as Harvey Weinstein. For example, a Starbucks worker commented to the Huffington Post *"I worked at Starbucks for three years during College. When one of my coworkers and I reported our shift manager for sexual harassment we were told we'd be laid off the following week"* (Agrawal, 2017). Thus, it seems that having perpetrators in a position of power might play an important role in how these events are adjudicated and the eventual impacts on victim and perpetrator outcomes.

Motivated by this, in Table 4 we consider heterogeneity in the impacts of workplace violence by the economic standing of victims relative to their perpetrators within the firm.²⁴ First, we interact the "treatment", i.e. an attack by a colleague, with whether the perpetrator is a manager for male-female crimes in column (1) and for male-male crimes in column (3) of Table 4. This exercise uses the coarse occupation variable in the data, and whether this variable indicates a managerial role for the perpetrator. Second, we interact the treatment of workplace violence with the difference in the income rank within the firm of the perpetrator and their victim, a broader measure of economic power differences between victim and perpetrator. This approach is possible because we observe the universe of workers in the firm and their labor market earnings, in addition to the perpetrator and his victim. For example, the income rank gap between a perpetrator at the 75th income percentile within the firm and a victim at the 25th income percentile would be 0.5. We report results of this second exercise in columns (2) and (4) of Table 4.

Table 4 shows that the economic power of perpetrators matters for the consequences of both male-female and male-male workplace violence. In both cases the employment impact on victims is greater when perpetrators are managers and there is a greater income gap between the victim and perpetrator. For perpetrators, the effect is the opposite: their employment rates are less severely impacted when they occupy relative positions of power within the firm. For male-female (male-male) crime, victims' employment rates fall by 5.6 (7.4) percentage points *more* when their

²³Widespread 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.

²⁴In Appendix Table A.6, we consider heterogeneity just by individual characteristics rather than differences across victims and perpetrators. Few significant patterns emerge.

perpetrator is a manager. However, perpetrators who are managers are 5.9 (13.3) percentage points *less* likely to be unemployed in the five years following an incident. Thus, power within the firm allows perpetrators to avoid more severe consequences, while their victims experience worse outcomes.

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

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

Notes: Table reports difference-in-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.

To test the degree to which economic power imbalances can explain the gaps in perpetrator outcomes for male-female versus male-male violence, we pool observations from male-female and male-male violence and interact the treatment of workplace violence with the gender of the

victim and either the income or managerial gap between victims and perpetrators. We estimate:

$$Y_{ibt} = \delta D_{ibt} + \beta D_{ibt} \times \text{Female Victim}_i + D_{ibt} \times \text{Power} + \gamma_t + \alpha_{ib} + \epsilon_{ibt} \quad (2)$$

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 workplace violence after the incident occurs. We again include individual-incident-year fixed effects, α_{ib} , and year fixed effects, γ_t . Power is either a dummy taking value 1 if the perpetrator is a manager, or the income rank gap between victim and perpetrator.

Appendix Table A.3 gives the results. For victims, the coefficient on the interaction between the treatment and a victim's gender is a precise zero. Consistent with Figure 4, 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 = 0.877).²⁵ However, as shown in Figure 4, there is a significant difference in the employment consequences for male perpetrators who attack female versus male colleagues prior to controlling for power differences. The magnitude of the interaction between the treatment and whether a victim is female reduces by 49.6% and becomes statistically insignificant once we control for the power imbalance between victim and perpetrator, albeit this interaction term is imprecisely estimated (p-value = 0.293). Thus, the smaller employment losses for male perpetrators with female victims in Figure 4 are partly 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.

3.4 Comparison to Non-Colleague Violence

This is a paper about violence between colleagues. Our main estimates focus on the impact of being attacked by a colleague relative to a counterfactual in which no violent event occurs: people should be able to go to work and never be attacked by a colleague. However, one might also be

²⁵Note that in the pooled regressions, age-time fixed effects are controlled for as per our baseline specification.

interested in the impact of being attacked by a colleague from work compared to the impact of being a victim of a similar crime, but when the perpetrator is not a colleague.²⁶ Similarly for perpetrators. For firms, we might be interested in whether there is a difference in outcomes when an employed perpetrator attacks a colleagues versus if they commit a crime against a non-colleague.

Thus, in this section we discuss results for this alternative "violent" counterfactual. For individuals, we identify matched controls from a set of people who are victims or perpetrators of the same crime category (i.e. menace), but who were not attacking or attacked by colleagues. We match exactly on employment status in the year preceding the crime and on crime type and then identify an individual's nearest neighbour on the basis of their age, education, and employment/income outcomes in the five years preceding an incident.²⁷ This violent counterfactual is not our preferred specification. Instead, our main specification above (a) captures the full repercussions of an assault by a colleague, not just the impact relative to other types of victimization and (b) captures the policy relevant counterfactual, namely being able to go to work without being assaulted.

However, the limited employment losses for perpetrators of male-female workplace violence is still evident when one considers the counterfactual of violence taking place between non-colleagues. Appendix Figure B.9 gives the difference-in-differences estimates for victims and perpetrators of male-female and male-male violence. The figure compares employment outcomes for perpetrators who attack a colleague with the employment outcomes of perpetrators who attack a non-colleague (and similarly for victims). For male-male violence, both victims and perpetrators suffer 4.5 percentage point larger employment losses than observationally equivalent individuals who experience or commit the same category of crime with a non-colleague. For male-female violence, however, while victims of colleague and non-colleague violence suffer identical labor market impacts, perpetrators of colleague violence suffer significantly smaller employment losses. Perpetrators employment rates are 3.1 percentage points greater over the five

²⁶Related is a small literature showing negative impacts of crime in general on victims in other contexts (Bindler and Ketel, 2022; Johnston *et al.*, 2018; Currie *et al.*, 2018; Koppensteiner and Menezes, 2021).

²⁷For this exercise we restrict our analysis to the four most frequent crime codes in our data (Table 1) to allow an exact match on crime type.

years following the incident compared to an observationally equivalent individual who commits a crime against a non-colleague (p-value=0.047). This is a striking finding given that violence against a colleague is likely more public to decision-makers within the firm than non-colleague violence and these decision-makers could naturally be expected to fire the perpetrator in order to preserve workplace culture.

3.5 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. In the remainder of this section, we implement a series of additional checks and a placebo test to further ensure that our main results capture the true consequences of violence between colleagues and are not confounded by alternative explanations.

Fuzzy Matching and Overfitting One might be worried about over-fitting on the pre-trends that could make the victim and matched control appear more similar than they actually are. To address this concern, we implement a fuzzy matching strategy. For this exercise, we match only on age, gender, education, employment, earnings, tenure, and industry in the year before a violent incident.²⁸ Results are reported in Appendix Figure B.14. The results are unchanged, and pre-trends are still statistically insignificant.

Additionally, in Table A.4 we report estimates that are robust to dropping two of the five pre-periods when identifying a nearest-neighbor match. The over-fitting robustness results are identical to our main results for both victims and perpetrators. Appendix Figures B.10 and B.12 show that the full event study estimates also look the same. In particular, pre-trends remain flat.

²⁸We also match on the cumulative crime record for defendants in the year before an incident.

Placebo Check The primary concern with our main results is that there is some time invariant or time varying unobservable difference between the victim/perpetrator and their matched controls, not picked up by the individual fixed effects, the time fixed effects, nor the matching.²⁹ One way to address this concern is to run a placebo specification where we artificially move our event to five years before the violent incident. Running our estimation strategy but estimating effects 5 years prior to workplace violence, when no crime occurs, should return a null-effect. This approach also addresses the concern that victims might be targeted because of their low income growth potential or that victims are different in some other unobservable way that is correlated with both being the victim of violence and the drops in employment we have documented.

In order to conduct this analysis, we repeat our matching procedure in the years before the new event. This is because, by construction, the nearest neighbor matches identified in our main specification were chosen to be observationally equivalent in the five years before violence, i.e. the post period in the placebo test. When we re-match, we only match 3 years prior to the placebo event year to avoid dropping many observations: ten years before a violent incident, a nontrivial share of the observations are in school rather than the workforce given the average ages of our sample and that individuals exit schooling in Nordic countries at later ages. Further, we restrict the set of potential matches to those who are employed at the point at which (future) violence occurs. Otherwise there would be a mechanical positive effect on employment rates of victims and perpetrators of workplace violence since they are, by construction, employed at the time of the future realized crime. In Appendix Figures B.11 and B.13 we find that there is no significant impact on victim nor perpetrator employment using this placebo, and we report the overall DiD estimates from this exercise in Table A.4.

Using Future Victims as the Counterfactual Victims might still be different in some other dimension not captured in the matching and individual fixed effects in our main results, not apparent in the pre-trends in the main analysis nor the fuzzy matching/over-fitting robustness

²⁹Additionally, for firms (which we examine later) there could be other observable and unobservable characteristics that results in changes in firm level recruitment and retention precisely after the violence between colleagues, but not due to that violence, and not addressed by the event study estimates using the matched control.

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

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

Other Possible Time-Varying Shocks The last possible alternative explanation for our results is that there is some other time-varying omitted variable not addressed in any of the above analysis that occurs at the time of the assault and explains both the assault and all of our main results: the employment impacts for both perpetrators and victims, the asymmetry between male-male and male-female violence, and the broader impacts on other employees at the firms we will document in the next part of the paper. It is difficult to come up with such a story that explains all of our results (instead of the obvious explanation, that one colleague attacked another), but

consider the following example.

Suppose that the perpetrator experiences a divorce that is highly disruptive and causes them to attack a colleague. If the divorce causes the assault, and the assault causes the impacts on employment and on the broader firm, then this would not be a problem for our results. For divorce to confound our results, it must be that the divorce would have caused: a) a drop in perpetrator employment; b) a drop in the victim's employment (even though the victim did not divorce); c) asymmetrical impacts for male-male versus male-female cases where the perpetrator's divorce leads to violence; and d) has broader impacts on other employees in the firm, as we will document. All four of these outcomes would need to occur even if the perpetrator did not attack a colleague, i.e. they would have occurred because of the divorce alone. We find it implausible that this is the casual narrative behind the effects we identify. Further, we find no change in cohabitation rates between our treatment and control groups, ruling out this specific challenge to our results.³⁰

Cohabitation As discussed in Section 2, the violent events we identify do not necessarily occur within the firm while at work. This is an advantage of our measurement of between colleague violence. For example, Harvey Weinstein famously assaulted colleagues in hotel rooms and other locations outside of the firm. An ideal measure would include all such incidents in the analysis. However, here we check the robustness of our results to excluding incidents where victim and perpetrators were cohabiting at the time of the crime or the year before. While such incidents are still between colleague violence, it is interesting to see if our main results are driven by them. There are no cases of male-male violence where individuals were cohabiting at the time of the event, so this analysis focuses on male-female violence. Appendix Figure B.15 shows that there is no statistically significant difference in our results when excluding intimate partner cases. The point estimates for perpetrators are indistinguishable, and the victim effects are larger, if not significantly so, when we exclude observations from victims and perpetrators who were cohabiting. Our results are thus not driven by domestic violence cases.

³⁰Formally, the p-value on the difference in changes in cohabitation status between the year before and of the violence effect is 0.43 for the full sample of workplace violence incidents, 0.53 for male-female violence and 0.95 for male-male violence.

4 Impacts on the Firm

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

Even if the overall size of the workforce does not change, the composition of workers at affected firms may change if workers with systematically different characteristics leave or join following a violent incident. For example, following an incident of male-female violence in the firm, other women in the firm may be more likely to leave (especially given the low separation rates for perpetrators). 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.³¹ 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.

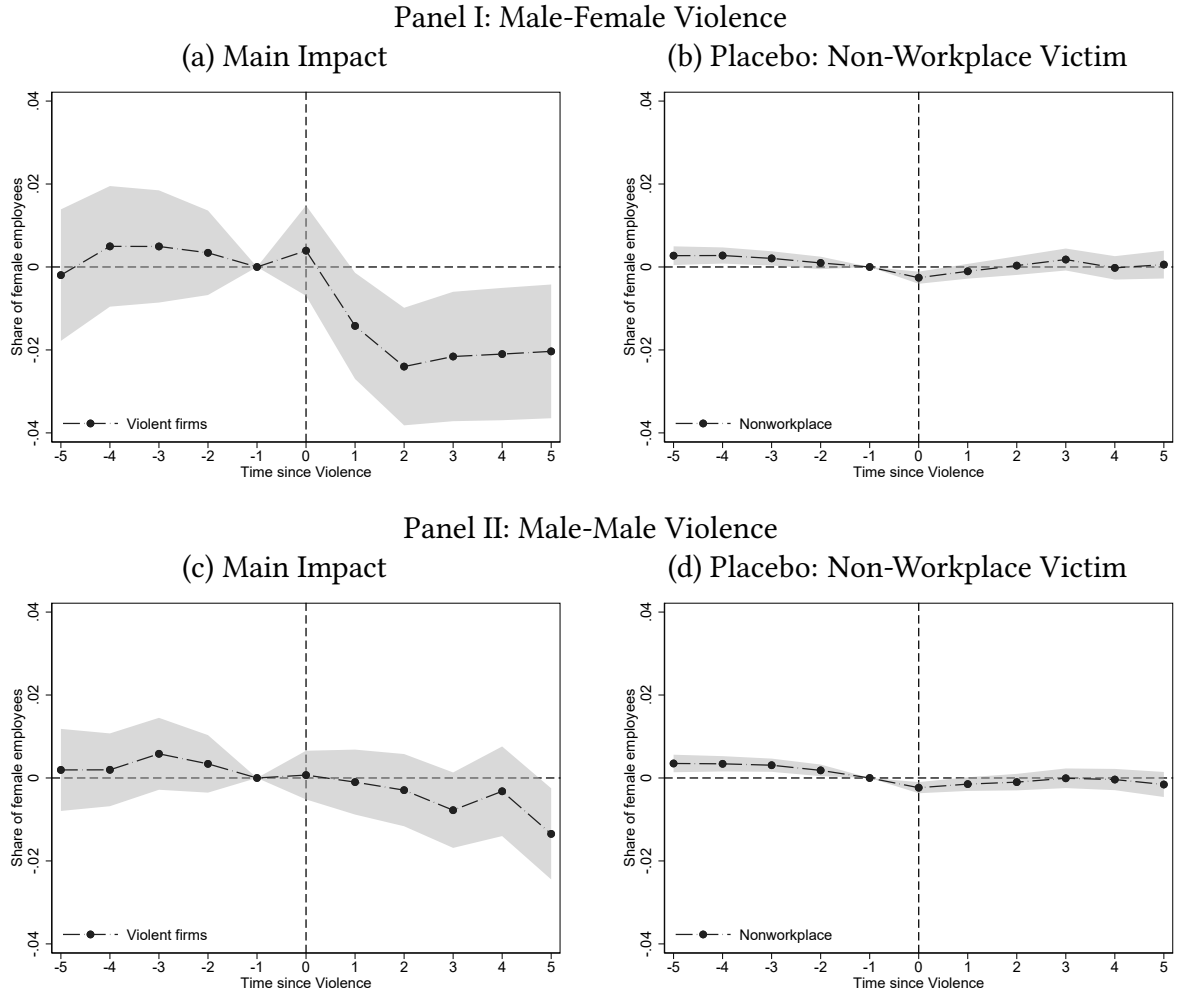
Figure 5 Panel I (a) demonstrates that after an incident of male-female workplace violence there is a significant decline in the proportion of women employed in firms where such violence took place relative to their matched control firm. The effect is quantitatively large and persistent: relative to the pre-incident mean, the share of women employed by the firm falls by 2 percentage points by five years after the incident. Figure 5 Panel II (b) demonstrates that this effect is isolated to firms where male-female violence occurs. There is no statistically significant effect of male-male violence on the share of women employed in the firm (and equivalently, no significant impact on the share of men employed).

Perhaps this decrease in the share of women employed by the firm is not due to between colleague violence. Instead, perhaps any time the firm employs a perpetrator of a violent crime,

³¹This 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.

this causes the share of women employed to decline, even when the victim is not employed by the same firm (i.e. she is employed by another firm or unemployed), because of this person's toxic presence. We explore this in a placebo test in Figure 5 Panels (b) and (d), where we estimate the impact of a violent crime where the perpetrator is employed by the firm, but the victim is not, on the share female within the firm. We find precise zeros for both male-female and male-male violence, with no impact on the broader firm from such cases. Thus, our results appear to be uniquely driven by the fact that one colleague attacked another colleague.

Figure 5: Impact on Share Female Employees in the Firm



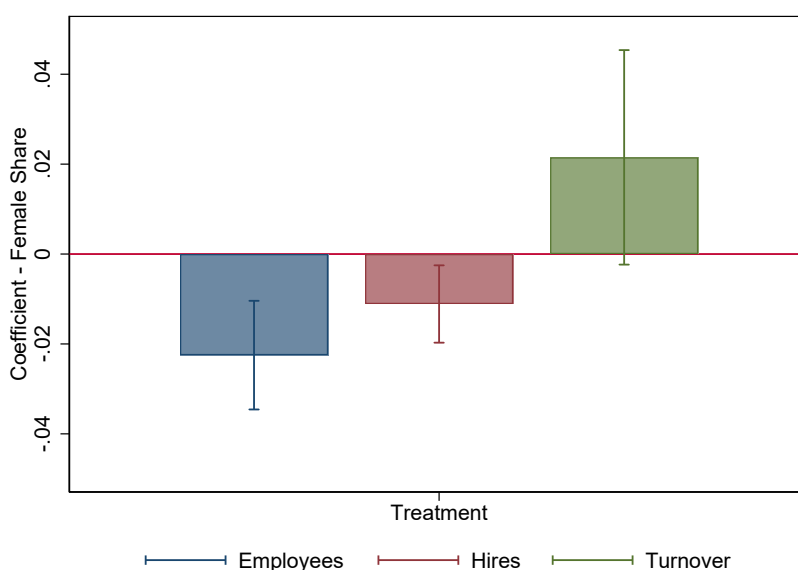
Notes: Left-hand-side figures show the impact of a violent incident between colleagues on the share of female workers at the firm in which both perpetrator and victim were employed at the time of the incident. The estimates use the matched control DiD design to identify effects 5 years before and 5 years 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. Horizontal axis displays time in years. Dashed vertical lines indicate the year of between-colleague violence. Panel I shows results restricting to incidents involving a male perpetrator and a female victim. Panel II shows results with male perpetrators and male victims. Standard errors are clustered at the firm level.

Next, we explore whether the decrease in the share of women in the firm is explained by more female employees leaving the firm or by the firm hiring fewer women after the incident. We find evidence that both dimensions are relevant in Figure 6.³² The proportion of women amongst firm leavers after male-female violence increases (p-value=0.077), while the share of

³²See Appendix Figure B.18 for the equivalent results for male-male crimes. As expected the figure shows no significant effects on any dimensions for male-male violence.

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

Figure 6: 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.

4.1 The Whisper Network and the Decline in Women Hires

Falls in the proportion of women hired by the firm could be driven by supply-side factors, i.e. women being less likely to apply for jobs at the firm, or by demand-side factors, i.e. the firm being less likely to hire women from a given set of applicants. We cannot observe the characteristics of applicants to the firm to analyze whether fewer women apply for positions after male-female violence. Instead, we investigate hiring from within the networks of existing employees to capture the potential influence of supply-side factors. Existing employee networks is

an obvious way in which potential future hires could find out about and avoid workplaces where one employee attacked another. A large literature shows that within-network hiring provides an important and high quality pool of potential applicants to the firm (Marmaros and Sacerdote, 2002; Bayer *et al.*, 2008; Beaman and Magruder, 2012; Brown *et al.*, 2016; Dustmann *et al.*, 2016). For example, Barwick *et al.* (2019) find that 38 (30) percent of workers in China (United States) find jobs through personal connections. Moreover, Hensvik and Skans (2016) show that firms are able to hire workers who are higher quality through referrals from existing highly productive workers.

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

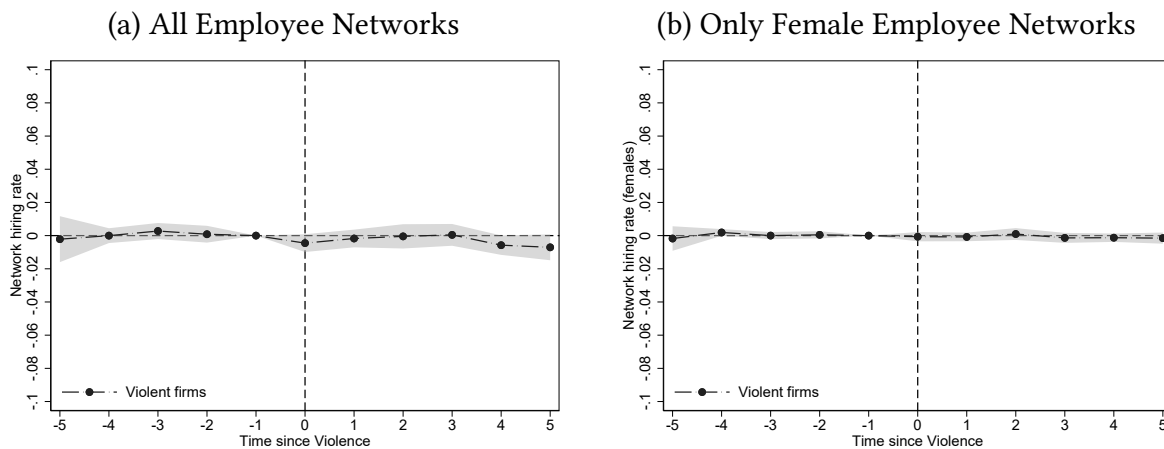
To test whether within network hiring decreases following a violent incident, we estimate the impact on the share of employees hired from previous colleagues of the firm's existing workers. To do so, we identify any person each existing employee worked with at the same time in some previous employment stint at some other firm in the 10 years prior to the incident year. Our definition of networks of past colleagues is consistent with that used in Hensvik and Skans (2016).

We report event study estimates of the impact of between-colleague violence on the share of new hires that come from within this network in Figure 7. 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, perhaps there is simply heterogeneity across networks. "Whisper networks" are generally assumed to function primarily between women. Thus, in Panel (b) of Figure 7 we re-estimate the main results but instead only look at the networks of women employees. We still find no impact on the hiring of future employees from existing women employees' networks.

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

Figure 7: Hiring from Within Employee Networks



Notes: Figure shows the impact of between-colleague violence on the share of hires that come from within the pool of past colleagues of current employees. Past colleagues consists of all current employees' past colleagues from the previous 10 years, similar to Hensvik and Skans (2016). Left-hand-side figure shows results for networks of all current employees, while right-hand-side figure restricts estimates to only the networks of existing female employees. All estimates reflect event-study estimates of equation 1 using the matched control to identify effects 5 years before and 5 years after a violent incident against a colleague. Standard errors are clustered at the firm level.

5 The Role of Managers

Firm differences in the management of colleague violence could mediate or accentuate the impact of violence on the wider workforce. Managers play an important role in determining the success of a firm (Bertrand and Schoar, 2003; Bloom *et al.*, 2007, 2013; Bandiera *et al.*, 2020; Gosnell *et al.*, 2020). Estimates suggest that differences in management account for 20% of the variation in productivity across plants (Bloom *et al.*, 2019). A smaller literature documents important heterogeneity in how male and female decision makers interpret and respond to negative (or positive) shocks, and how the gender of the individual responsible for the shock might change the response of the manager (Benson *et al.*, 2021; Chakraborty *et al.*, 2021; Sarsons, 2017).³³ Most closely related to this paper, Egan *et al.* (2022) find that following incidents of financial misconduct by financial advisers, women who commit such misconduct are more likely to be fired. They find that this disparity in the consequences for financial misconduct by gender are driven entirely by male-managed firms.

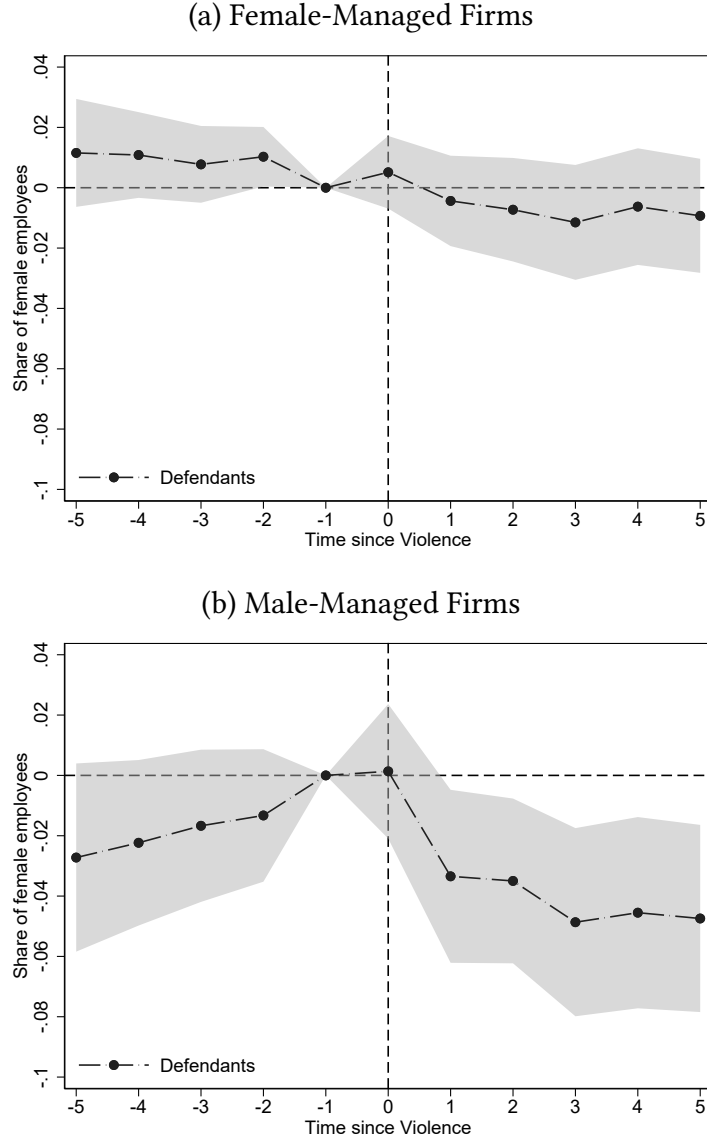
Motivated by these facts, we consider heterogeneity in the impact of male-female violence on the share of female workers in the firm by the proportion of women in decision-making positions within the firm. Following Bender *et al.* (2018), we identify workers in the top 20% of earners in the firm as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

Figure 8 shows that while there is a significant decline in the share of female employees following an incident of male-female violence in male-managed firms, we see no significant impact on the share of female employees for female-managed firms. The persistence in the fall of women

³³See also the literature on how female leadership impacts policy (Ford and Pande, 2011; Bertrand *et al.*, 2019).

for male-managed firms is particularly striking, with significant negative effects lasting at least five years, compared to no significant effects and estimates close to zero for all years after the incident for female-managed firms. The overall impacts are also large, with an almost 6 percentage point decline in the share of women employed in male-managed firms by five years after male-female violence. This effect is quantitatively significant relative to the baseline share of women employed in these firms of 24.1%. In Table 6 we estimate the role of male versus female management in a regression framework. Column (1) interacts the treatment variable of workplace violence with a dummy variable indicating whether the firm is female-managed. We find that female management is associated with significantly more women in the firm following male-female workplace violence (p -value=0.011), such that the gender composition of the firm remains unchanged.

Figure 8: 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 (top figure) versus male-managed firms (bottom figure). We define management as "male" if the share of men in the top 20% of earners is above the median share, and "female" otherwise. Standard errors are clustered at the firm level.

5.1 What Do Female-Managed Firms Do Differently?

How does female management mediate the impact of male-female violence on the gender composition of the firm? A surprising finding of Section 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. Women employees might feel more comfortable remaining in a firm where perpetrators of male-female violence face consequences for their actions. Moreover, managers who show a willingness to hold workers who perpetrate violence to account might also be less likely to punish women in general following male-female violence, i.e. they may be less likely to shift their hiring towards men following male-female violence. For example, a manager who didn't fire the perpetrator might have to worry about the perpetrator attacking future women. One way to reduce that possibility is to simply stop hiring women, as we found occurs following male-female violence (see Figure 6).

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

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

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

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

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

In Table 6, we jointly examine the impact of perpetrators losing their job and of female management on the share of women employed in the firm. To capture perpetrator job loss, we introduce a dummy variable equal to 1 if the perpetrator loses their job in the post-violence period. Col-

umn (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 ($p\text{-value}=0.068$), although this does not fully negate the full impact of the violence. It is interesting that it is only the interaction of female management and a perpetrator losing their job that mediates the impact of male-female violence on the wider workforce ($p\text{-value}=0.069$). Female-managed firms where the perpetrator remains employed do not have significantly different outcomes to male-managed firms, and male-managed firms where perpetrators leave still face significant falls in the proportion of women employed.

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

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

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

6 Discussion

Our results have a number of implications. First, female victims of workplace violence have few economic incentives to report violence at work. Even in the relatively severe cases reported to the police in our data, the male perpetrator experiences relatively small labor market costs for his actions. This is consistent with the vast under-reporting of workplace harassment and abuse suggested by survey data.³⁴ A major, known problem in preventing harassment at work is that

³⁴"Based on anonymous survey responses, no fewer than 1 in 28 U.S. workers report having been victimized by workplace sexual harassment annually. Yet only 1 in 11,000 workers file a formal sexual harassment charge with the Equal Employment Opportunity Commission (EEOC), the agency tasked with enforcing all federal anti-discrimination laws." (Dahl and Knepper, 2021, p.1)

victims rarely report the problem to their employer (Magley, 2002; Boudreau *et al.*, 2022). Women under-reporting harassment and violence at the hands of a colleague (and in particular one's manager) is easily reconciled with the comparative lack of career consequences for perpetrators of male-female violence we have documented.

Second, given that under-reporting is common, we are likely only observing a small fraction of all cases of workplace violence. As described in Section 2, just 10% of physical assaults are reported to the police in Finland, with lower reporting rates for crimes considered less serious by the victim (EU Agency for Fundamental Rights, 2015; European Institute for Crime Prevention & Control, 2009). Conservatively, this implies that the incidence of workplace violence is at least 10 times larger than can be documented by police reports. At the same time, under-reporting and selective reporting is relevant for the external validity of our results. While we provide the first evidence of the impacts of workplace violence on perpetrators, victims, and the broader firm, we can only do so for the (likely) more severe cases reported to police. We might not expect to see quite as large of impacts on victims, perpetrators, and the firm from less severe abuse by colleagues.

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

perpetrator, preventing informal networks like "whisper networks" from solving these issues.

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

7 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 impacts on victims and perpetrators. However, male perpetrators of male-female violence experience less severe repercussions compared with perpetrators of male-male violence.

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

We also show that male-female violence has broader implications for women in the firm in general and not just for the female victim. Following male-female violence, firms become signif-

icantly more male, with no such repercussions following male-male violence. This is explained both by a reduction in the share of female new hires as well as women leaving the firm. We find that the drop in new female hires is unlikely to be a supply-side phenomenon as there is no impact on hiring within employee networks. This result suggests that informal "whisper networks" are unlikely to resolve these issues.

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

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

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Online Appendix

A Additional Tables

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

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

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

Table A.2: Descriptive Regressions

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

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

Table A.3: Pooled Employment Effects & Power Discrepancies

	Dependent Variable: Victim Employment	
	(1)	(2)
Treatment	-0.0714 (0.0102)	-0.0521 (0.0116)
Treatment*Female Victim	-0.0019 (0.0124)	0.0012 (0.0126)
Treatment*Perp is Manager		-0.0529 (0.0247)
Treatment*Income Gap		-0.0287 (0.0132)
Observations	57456	57456
	Dependent Variable: Perpetrator Employment	
	(1)	(2)
Treatment	-0.0842 (0.0104)	-0.1090 (0.0129)
Treatment*Female Victim	0.0270 (0.0127)	0.0134 (0.0127)
Treatment*Perp is Manager		0.0709 (0.0151)
Treatment*Income Gap		0.0832 (0.0126)
Observations	58086	58086
Year fixed effects	✓	✓
Time since crime fixed effects	✓	✓
Individual fixed effects	✓	✓
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. See Section 3.3 for more details.

Table A.4: Employment Effects for Victims and Perpetrators & Robustness Checks

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

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

Table A.5: Employment and Income Effects for Victims and Perpetrators

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

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

Table A.6: The Effect of Workplace Violence on Employment for Male-Female Violence: Heterogeneity by Individual Characteristics

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

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

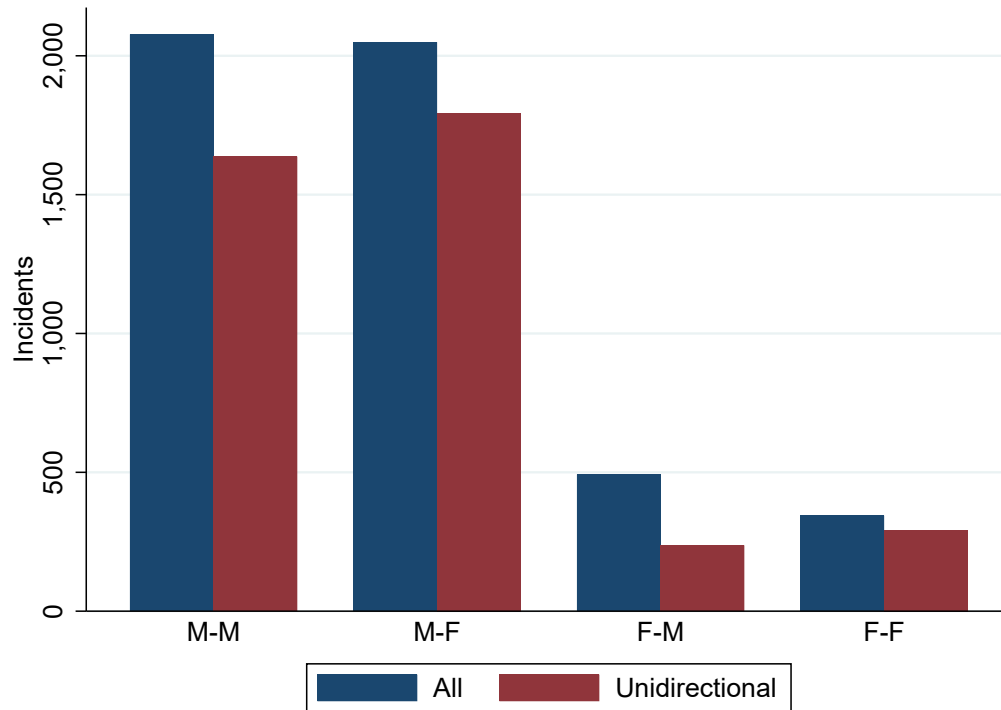
Table A.7: Impact of Workplace Violence on Firm Outcomes

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

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

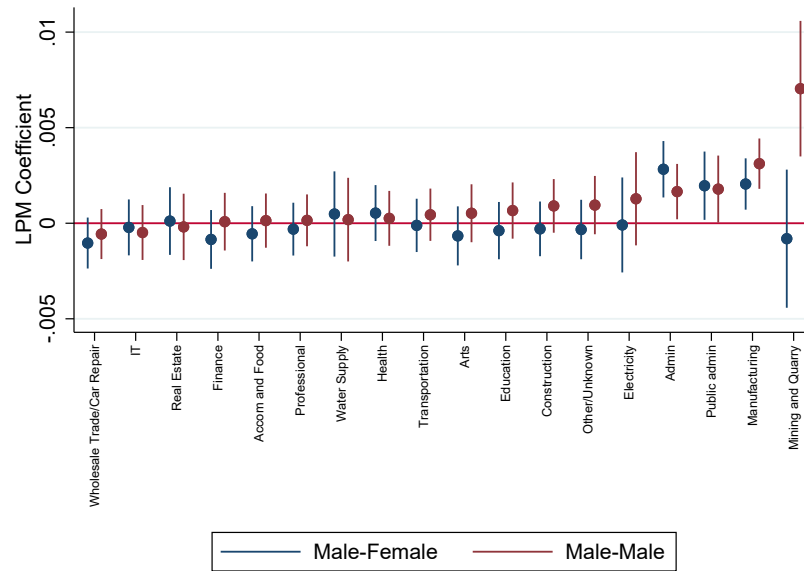
B Additional Figures

Figure B.1: Gender Breakdown of Workplace of Cases of Workplace Violence



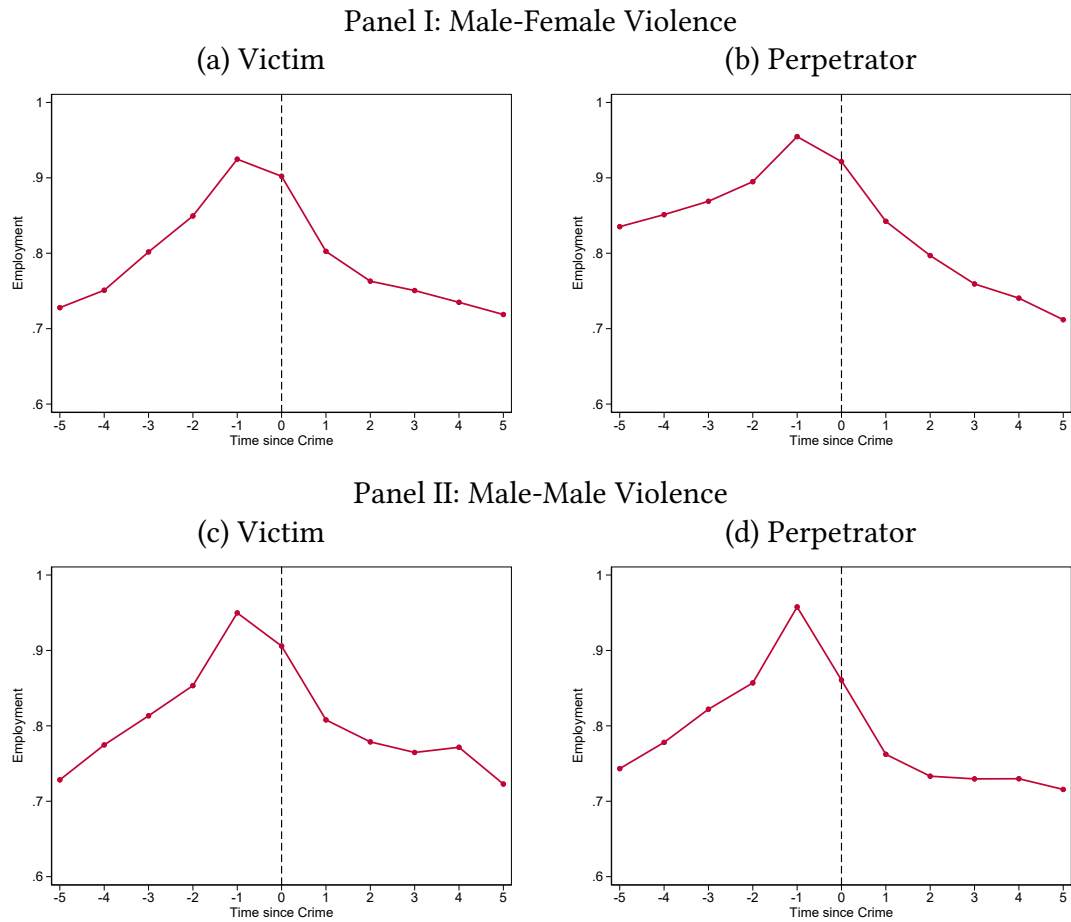
Notes: Figure shows the number of cases within each violence category in blue bars for: male-male, male-female, female-male, and female-female violence where, for example, male-female violence cases consist of a male perpetrator and a female victim. In red bars we indicate the number of cases where the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

Figure B.2: Industries Where Between Colleague Violence Occurs



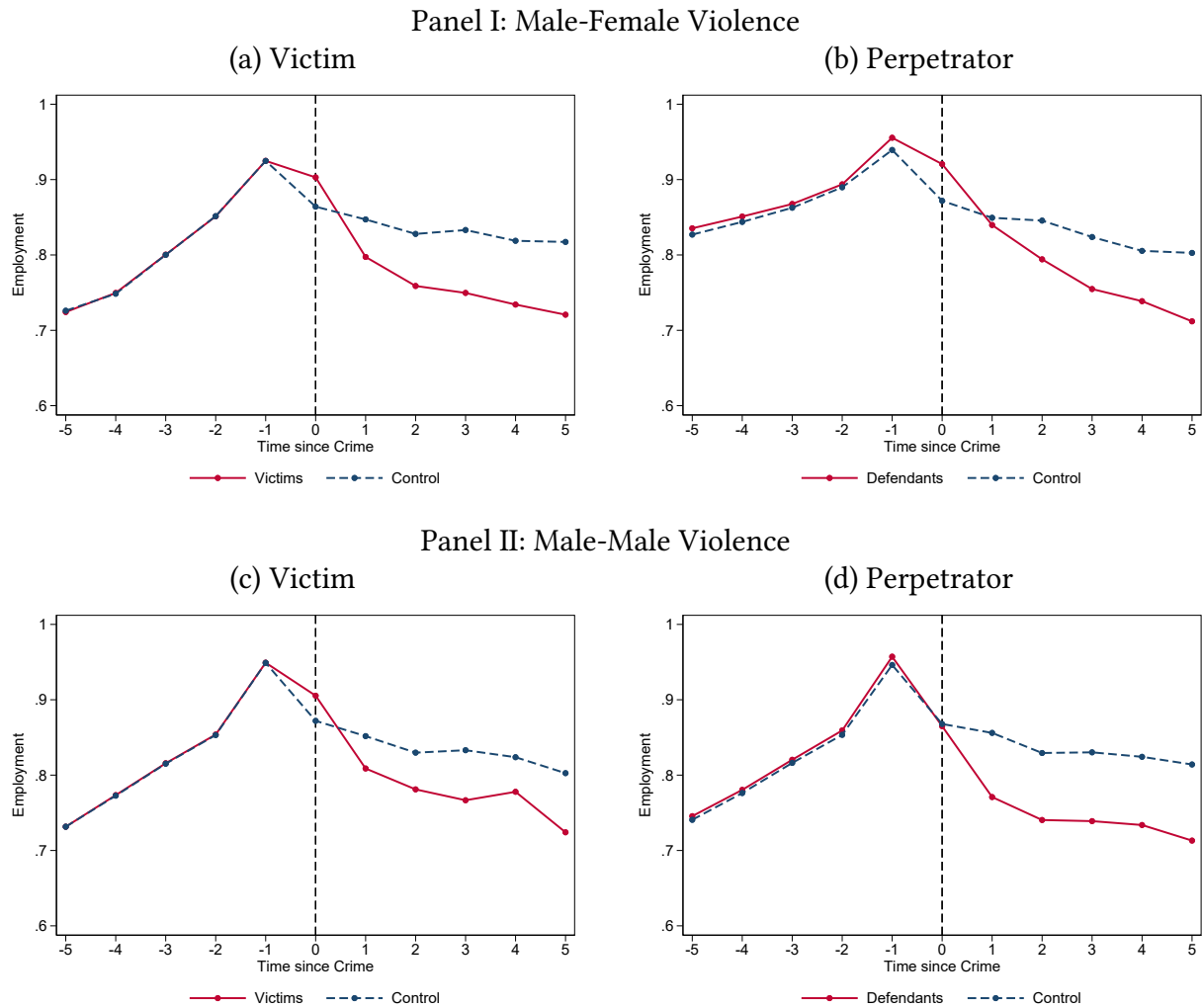
Notes: Figure reports estimates of LPM regressions where we regress industry dummies on dummies indicating whether male-female (in blue) and male-male (in red) violence between colleagues took place.

Figure B.3: Descriptive Raw Mean Employment of Victims and Perpetrators Before and After Violence



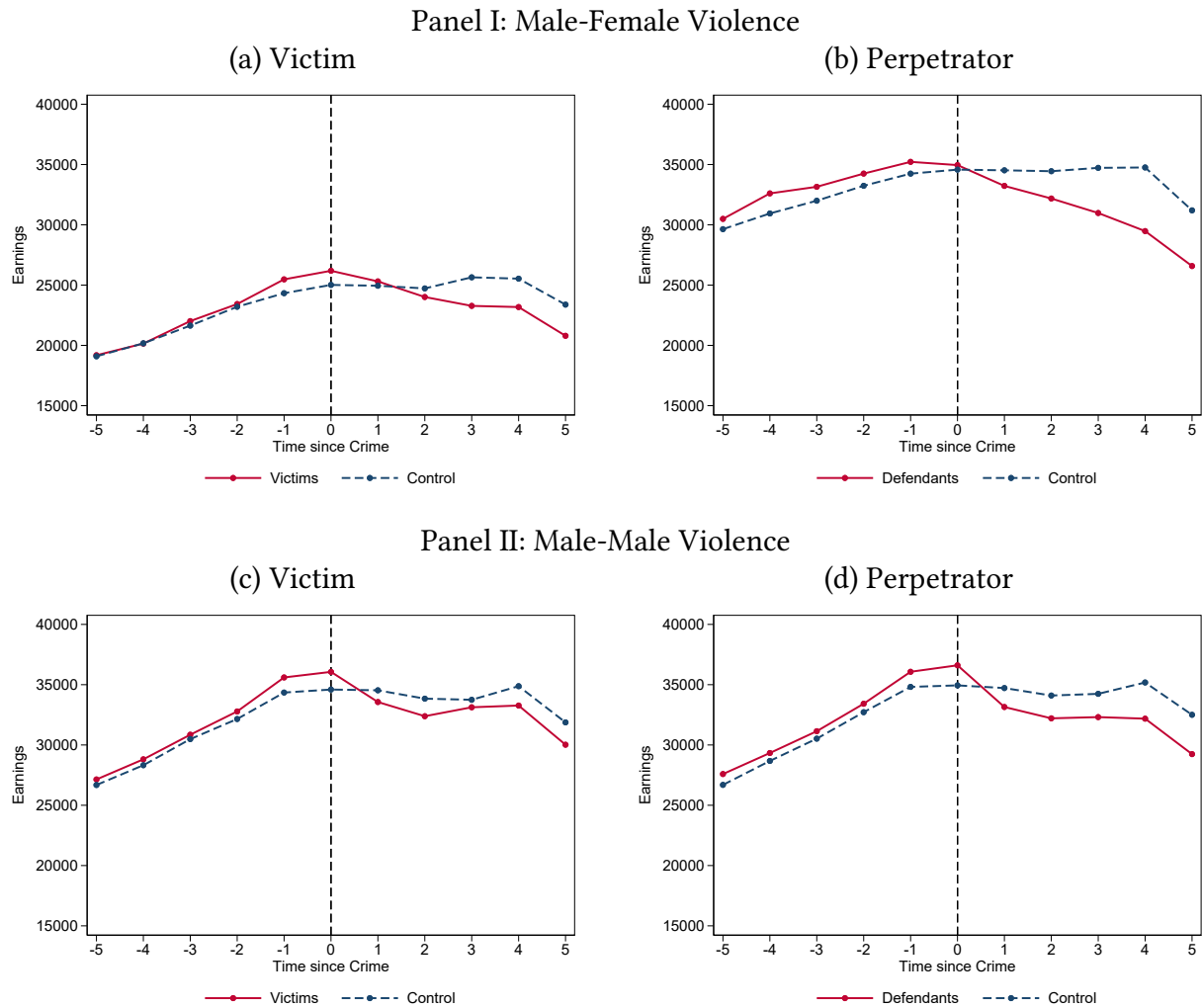
Notes Each figure reports the average employment of the victim (left-hand side) or perpetrator (right-hand side) before and after a violent incident between colleagues that results in a police report. First row reports effects for male-female violence. Second row reports effects for male-male violence. Employment is measured at the end of the year.

Figure B.4: Raw Patterns of Employment for Victims and Perpetrators (and Their Matches) Before and After Colleague Violence



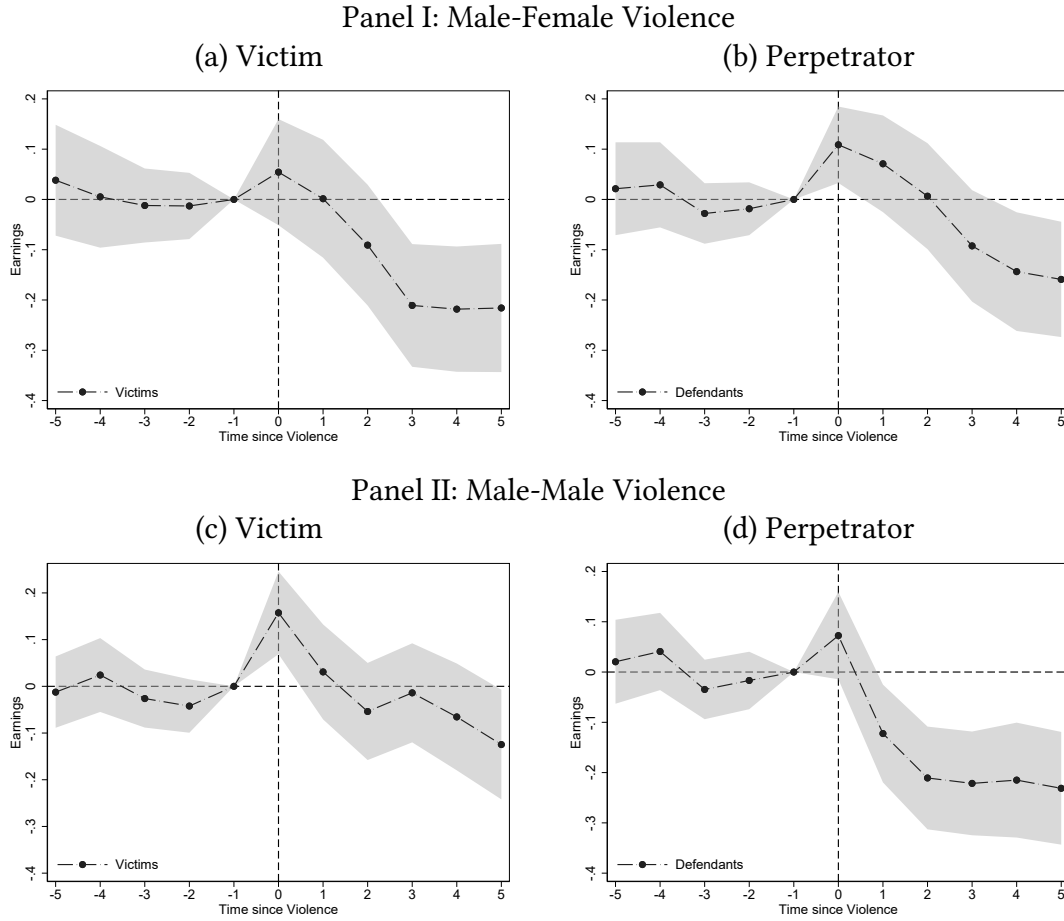
Notes: Panel I shows average employment for male-female violence for victims (left) and perpetrators (right). Panel II shows the same but for male-male crimes. Victim and perpetrator averages are depicted in the red lines and their matched controls are depicted in dashed blue lines. Raw averages reported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

Figure B.5: Raw Patterns of Income for Victims and Perpetrators (and Their Matches) Before and After Colleague Violence



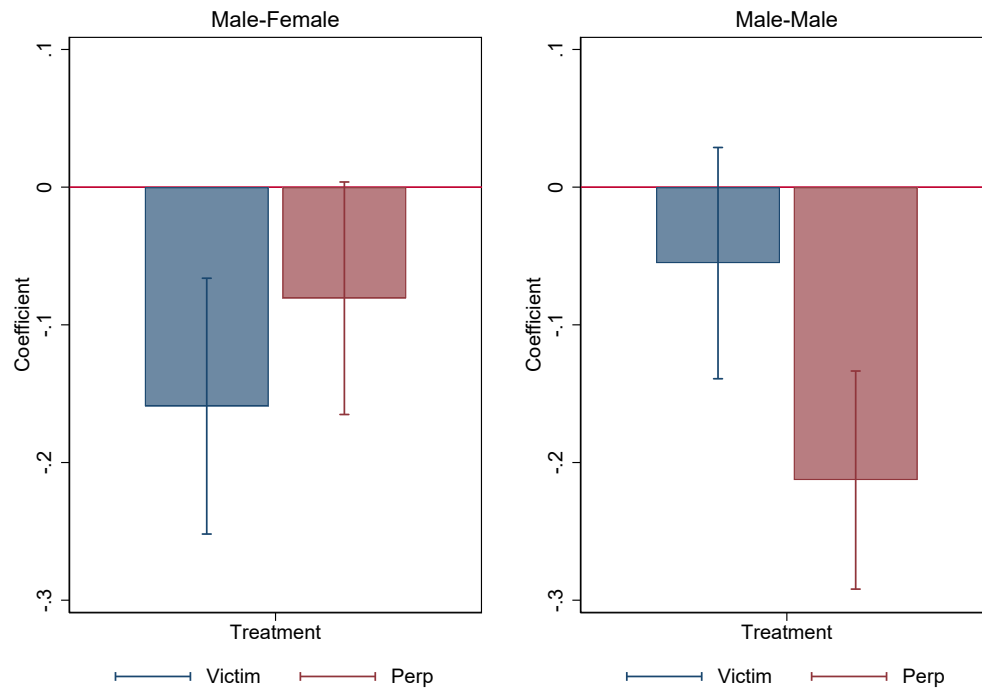
Notes: Panel I shows average income for male-female violence for victims (left) and perpetrators (right). Panel II shows the same but for male-male crimes. Victim and perpetrator averages are depicted in red lines and their matched controls are depicted in dashed blue lines. Raw averages reported 5 years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

Figure B.6: Impact of Colleague Violence on Income of Victims and Perpetrators



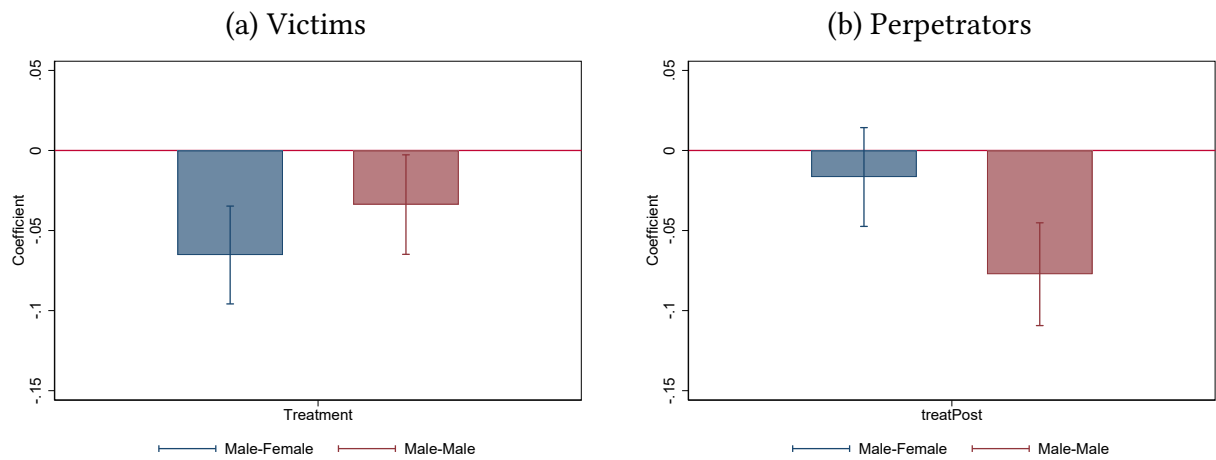
Notes: Each figure reports the impact of a violent incident between colleagues that results in a police report on income of the victim (left-hand side) or perpetrator (right-hand side). Income is measured as the income in year t as a fraction of the income in the year before the event. First row reports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

Figure B.7: Asymmetry in Impacts of Workplace Violence on Percent Income Changes



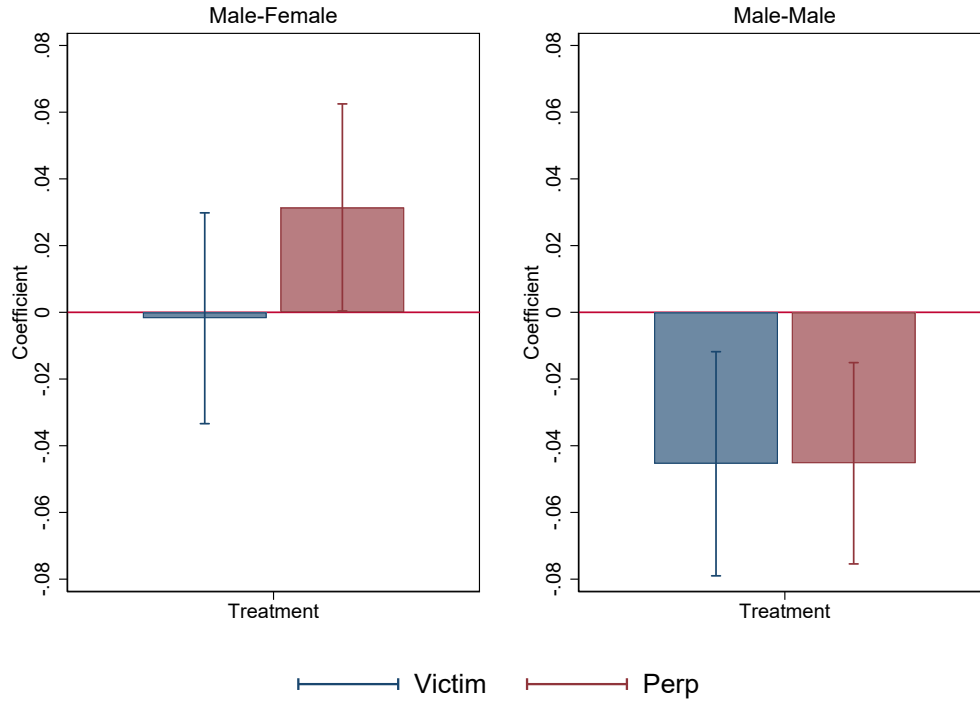
Notes: Figure reports estimates of δ_t obtained using Equation (1) where we collapse into a pre- and post-period to recover difference-in-differences estimates. Left-hand figure reports DiD estimates for male-female crimes for victims (in the blue bar on the left) and perpetrators (in the red bar on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

Figure B.8: Workplace Transitions



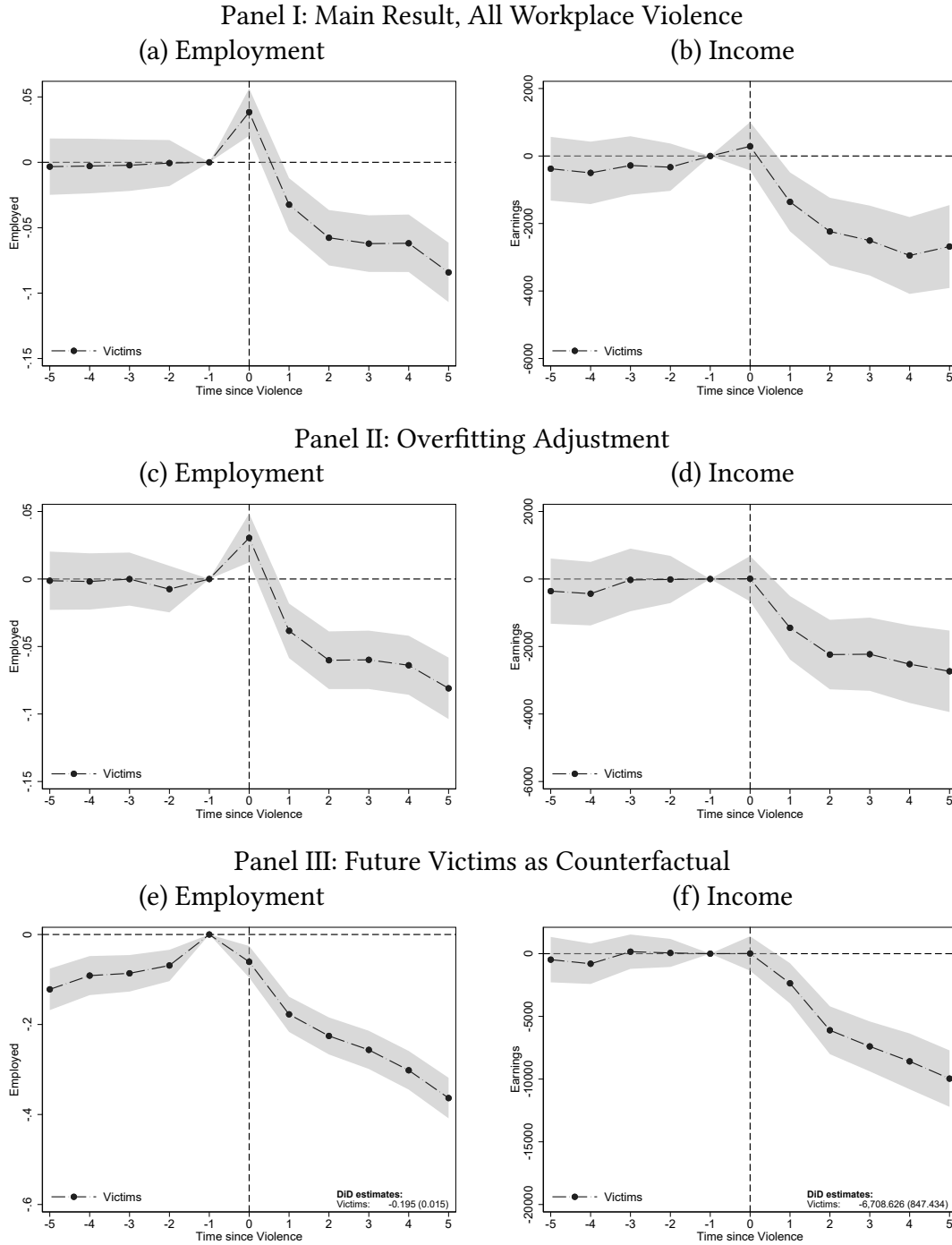
Notes: Figure (a) shows the DiD estimates with the dependent variable equal to whether the victim of workplace violence is in the same firm for male-female violence (left bar in blue) and for male-male violence (right bar in red). Figure (b) shows the DiD estimates with the dependent variable equal to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

Figure B.9: Comparing Workplace and Non-Workplace Violence Impacts



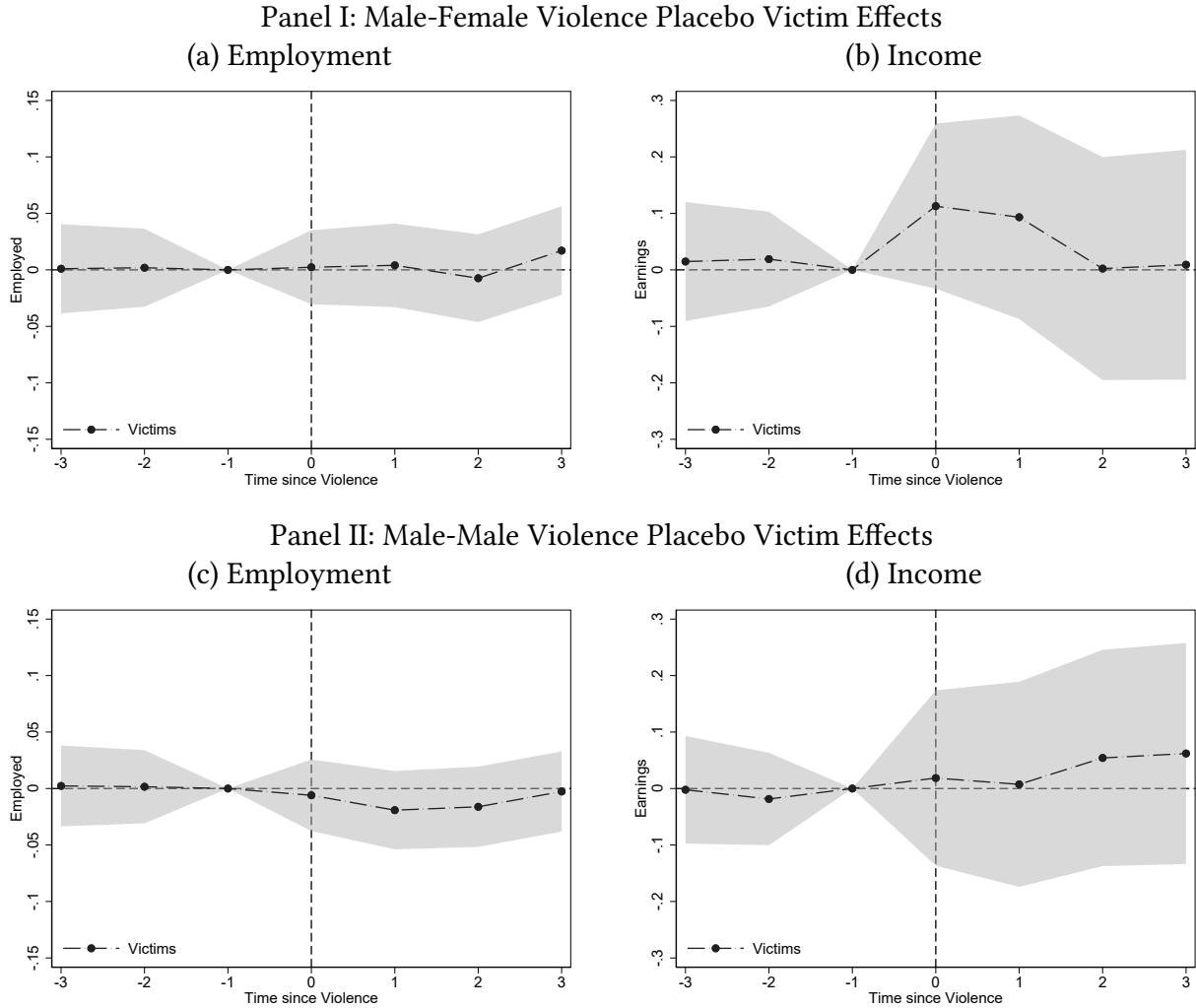
Notes: Figure reports estimates of δ_t obtained using Equation (1) where we collapse into a pre- and post-period to recover difference-in-differences estimates. However, unlike the main estimates which uses a nearest neighbor match who is not also a victim (or perpetrator when estimating perpetrator impacts) of a crime, in this analysis we compare outcomes to a nearest neighbor match who was also a victim (or perpetrator) of one of the same main types of crimes in Table 1. Left-hand figure reports DiD estimates for male-female crimes for victims (in the blue bar on the left) and perpetrators (in the red bar on the right) compared with the impacts for non-workplace victims and perpetrators. Similarly for male-male crimes in the right-hand figure. 95% confidence intervals depicted in whiskers around the estimates. Employment is measured at the end of the year. See Sections 2 and 3.4 for more details. Standard errors are clustered at the individual level.

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



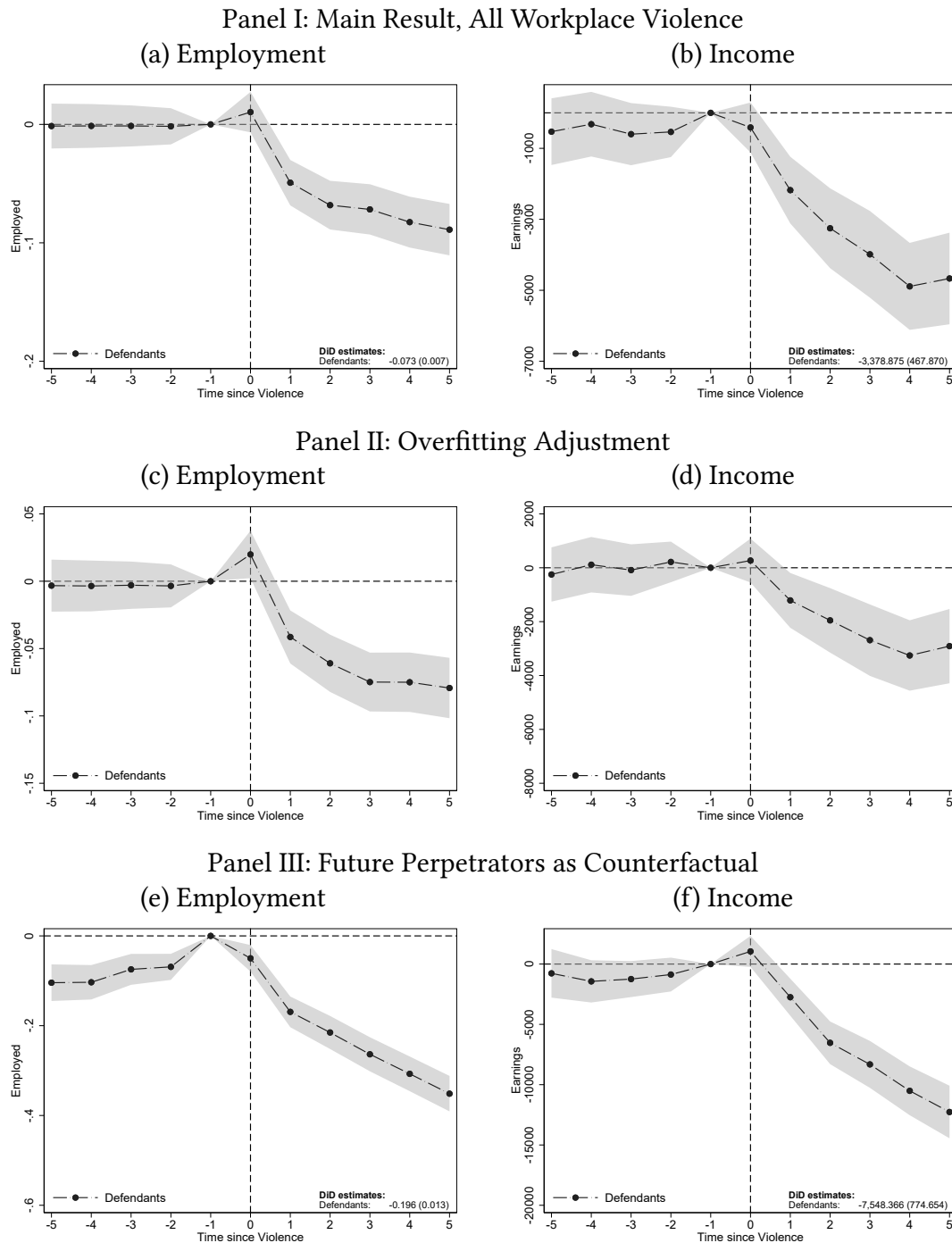
Notes: Each figure reports event study estimates of the impact of a violent incident between colleagues that results in a police report on victim employment (left-hand side) or income (right-hand side). Panel I repeats the main estimates for all workplace crimes. Panel II reports results where we address the possibility of overfitting by dropping two of the pre-period years when matching. Panel III reports estimates using future victims who are attacked by a colleague in a year beyond the post period in a stacked event study as the control to identify effects 5 years before and 5 years after a violent incident. Specifically, we take victims from 2014-2016 as the counterfactual for victims from 2006-2008, thus treatment effects do not overlap in the post period. Employment and income are both measured at the end of the year, and income is measured in Euros. See Section 3.5 for more details. Standard errors are clustered at the individual level.

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



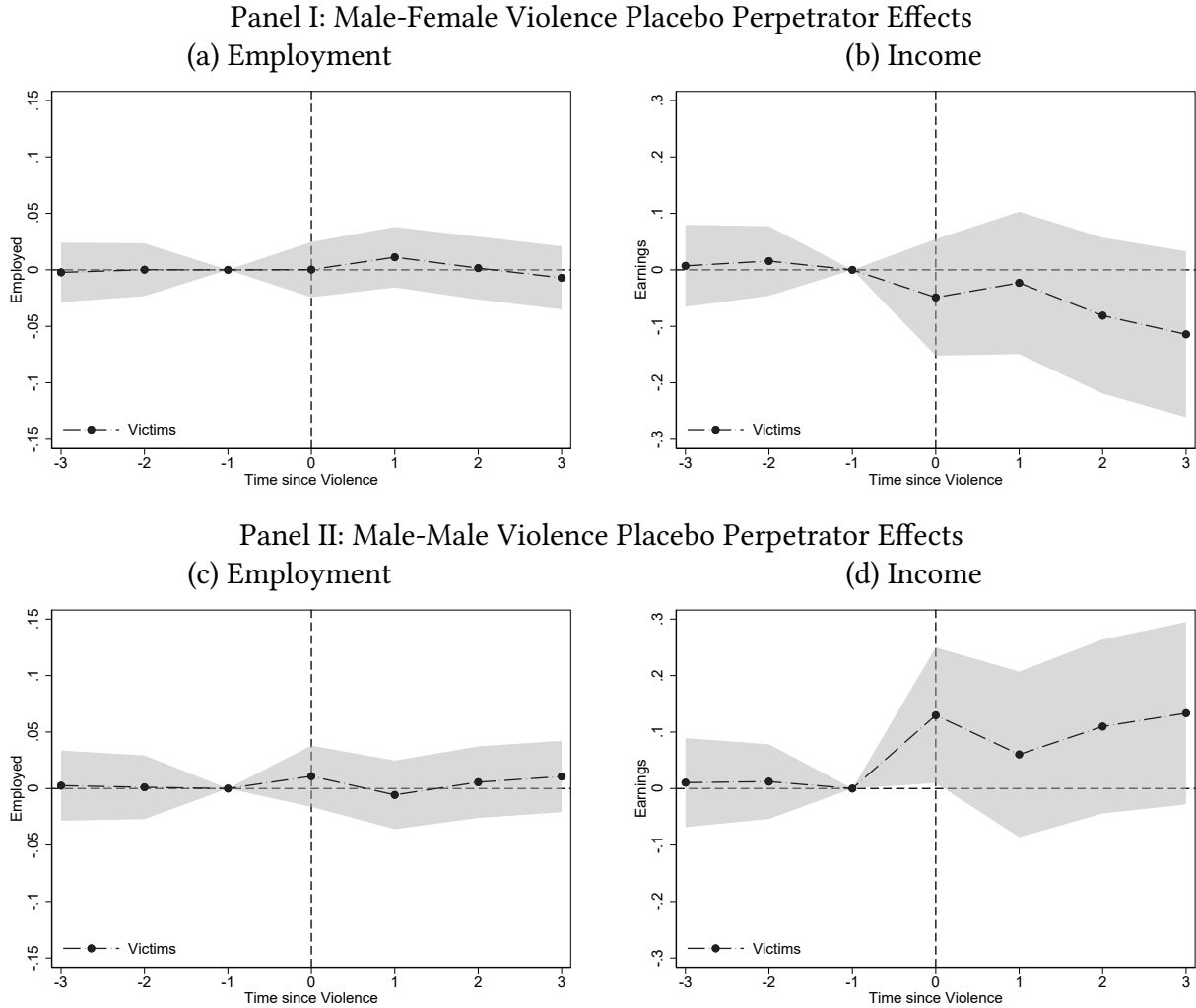
Notes: Panel I shows the impact of a placebo event 5 years prior to a male-female violent incident that results in a police report on the female future victim's employment relative to her matched control in subfigure (a) and the female future victim's income in subfigure (b). In Panel II we report the placebo results for male victims of male-male crimes. Employment and income are both measured at the end of the year, and income is measured in Euros. The placebo exercise moves the "event" line 5 years prior to the actual violent event, redoes the nearest neighbor matching to find a counterfactual, and re-estimates equation 1 to calculate effects 3 years before and after this placebo event. For more details, see Section 3.5. Standard errors are clustered at the individual level.

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



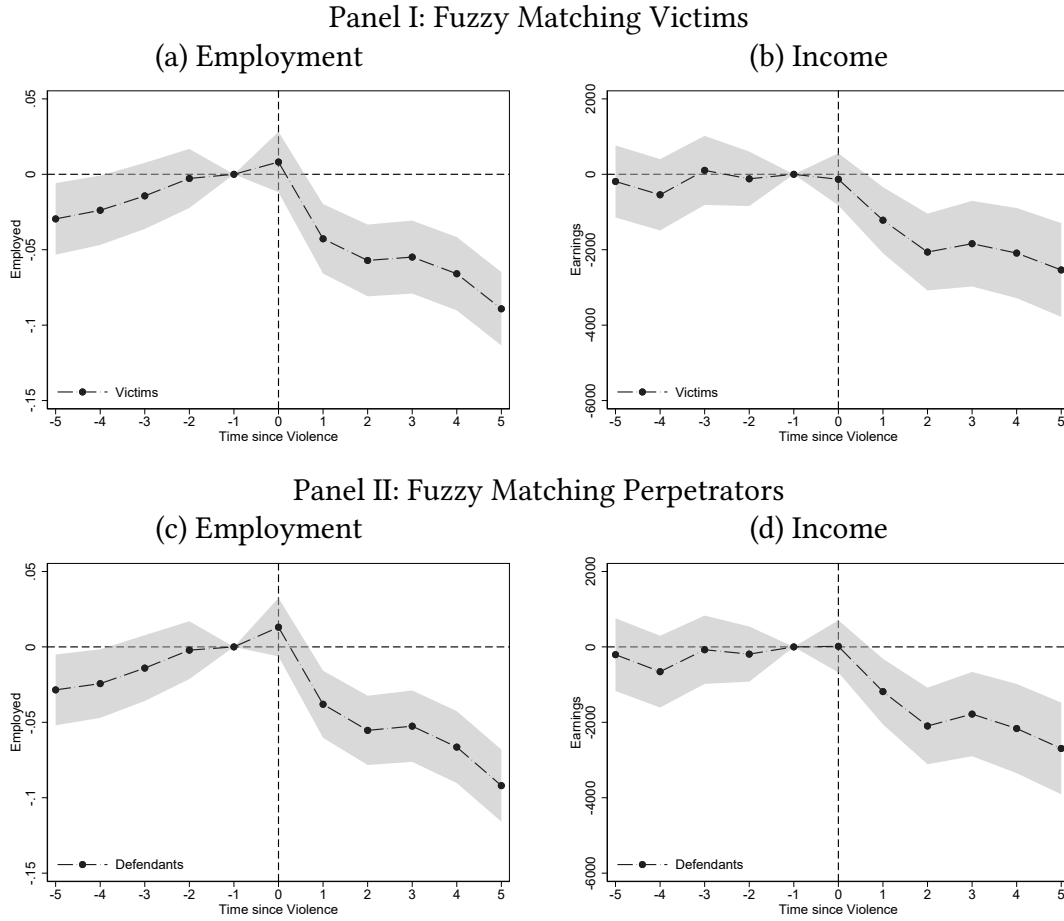
Notes: Each figure reports event study estimates of the impact of a violent incident between colleagues that results in a police report on perpetrator employment (left-hand side) or income (right-hand side). Panel I repeats the main estimates for all workplace crimes. Panel II reports results where we address the possibility of overfitting by dropping two of the pre-period years when matching. Panel III reports estimates using future perpetrators who attack a colleague in a year beyond the post period in a stacked event study as the control to identify effects 5 years before and 5 years after a violent incident. Specifically, we take perpetrators from 2014-2016 as the counterfactual for perpetrators from 2006-2008, thus treatment effects do not overlap in the post period. Employment and income are both measured at the end of the year, and income is measured in Euros. See Section 3.5 for more details. Standard errors are clustered at the individual level.

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



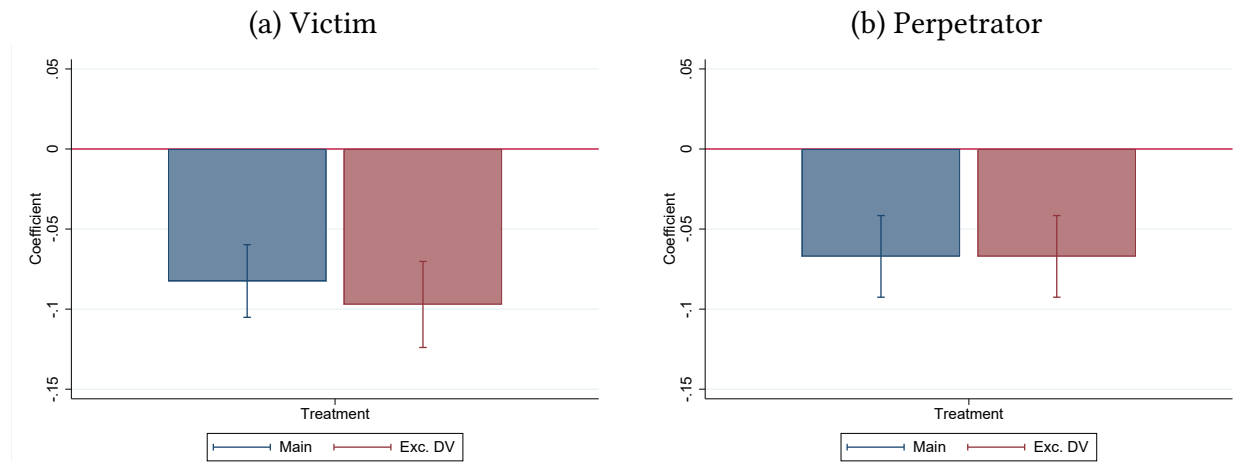
Notes: Panel I shows the impact of a placebo event 5 years prior to a male-female violent incident that results in a police report on the male future perpetrator's employment relative to his matched control in subfigure (a) and the male future perpetrator's income in subfigure (b). In Panel II we report the placebo results for male perpetrators of male-male crimes. Employment and income are both measured at the end of the year, and income is measured in Euros. The placebo exercise moves the "event" line 5 years prior to the actual violent event, redoes the nearest neighbor matching to find a counterfactual, and re-estimates equation 1 to calculate effects 3 years before and after this placebo event. For more details, see Section 3.5. Standard errors are clustered at the individual level.

Figure B.14: Robustness of Victim and Defendant Impacts to Fuzzier Matching



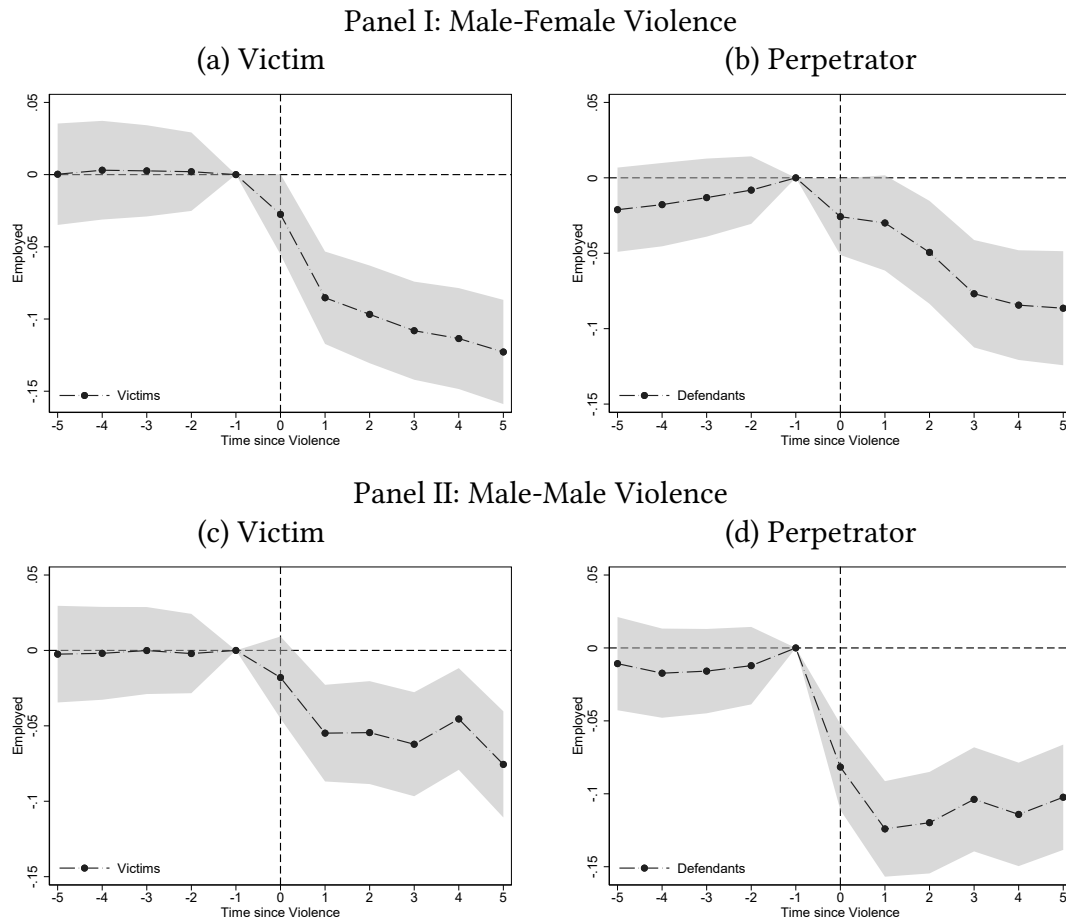
Notes: Each figure reports event study estimates of the impact of a violent incident between colleagues that results in a police report on victim employment (left-hand side) or income (right-hand side). Panel I repeats the main estimates for all workplace crimes. Panel II reports fuzzy matching results for victims. Panel III reports fuzzy matching results for defendants. Employment and income are both measured at the end of the year, and income is measured in Euros. See Section 3.5 for more details. Standard errors are clustered at the individual level.

Figure B.15: DiD Employment Impacts Excluding Domestic Violence Cases from the Sample for Male-Female Violence Only



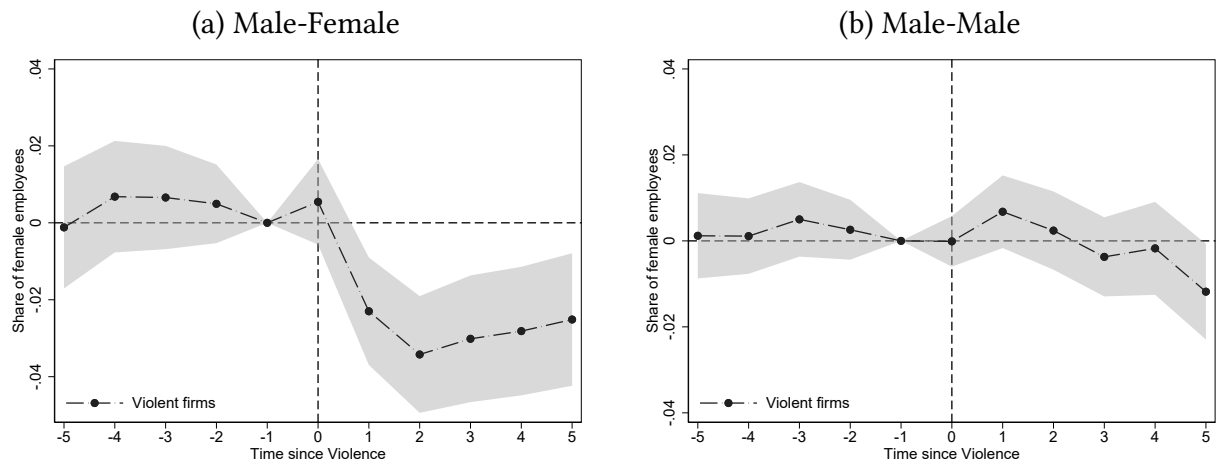
Notes: Figure (a) shows the DiD estimates with the dependent variable equal to employment for our main specification on the full sample (blue) and when we drop cases where the victim and perpetrator were cohabiting the year before or of the incident. Panel (b) gives the same for perpetrators. Standard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

Figure B.16: Robustness of Victim and Defendant Impacts to Only Including Colleagues from December Prior to the Event



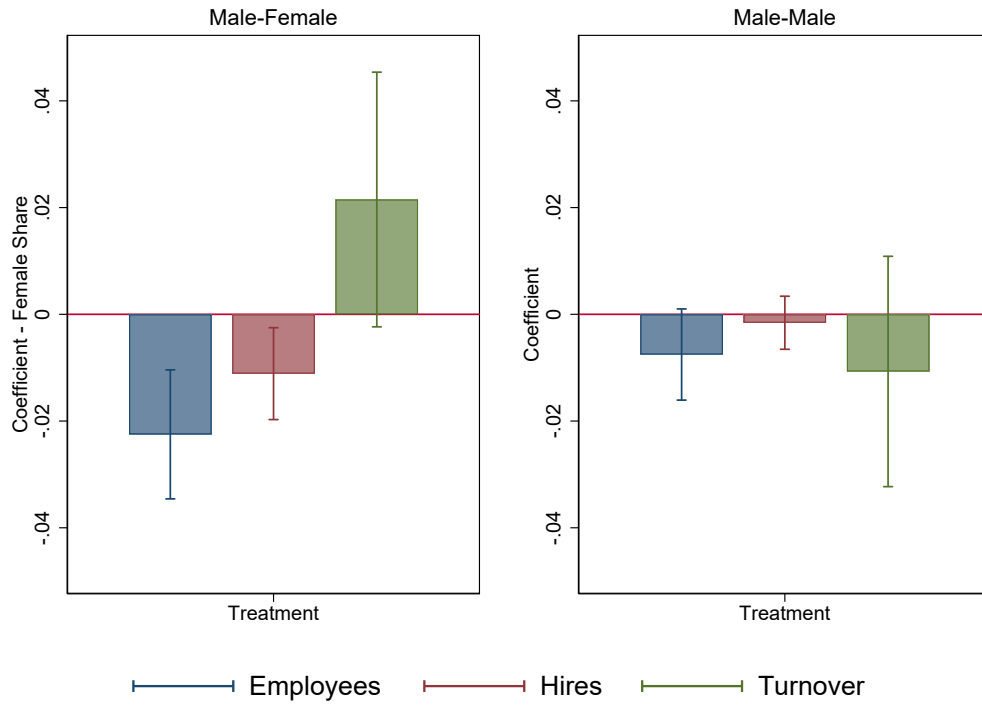
Notes: Each figure reports the impact of a violent incident between colleagues that results in a police report on employment of the victim (left-hand side) or perpetrator (right-hand side). First row reports results for male-female violence. Second row reports effects for male-male violence. The estimates use the matched control to identify effects 5 years before and 5 years after a violent incident against a colleague (see equation 1). Employment is measured at the end of the year. The only difference between these Figures and Figure 3 in the main text is that here we only include events where the colleagues worked together the December prior to violence, excluding the minority of cases where we only observe colleagues working together the December after the incident. See Section 2 for more details. Standard errors are clustered at the individual level.

Figure B.17: Estimates of the Impact on Share Female Workers: Excluding Perpetrators and Victims



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

Figure B.18: Individual Components of the Drop in Share Female Employees



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