Breaking Silence: How Intimate Partner Violence And Reporting Shape Later Life Outcomes*

Harrison Chang
University of Toronto

Shiau-Fang Chao
National Taiwan University

Kuan-Ming Chen
National Taiwan University

Ming-Jen Lin
National Taiwan University

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Abstract

We study the impact of Intimate Partner Violence (IPV) on various aspects of the victims' lives throughout the course of violence, including their labor market performance, marital decisions, fertility decisions, and health outcomes. Our data consists of the universe of administratively reported IPV cases primarily from hospitals, police, and helplines in Taiwan from 2012 to 2018. We distinguish the violence effect and the report effect. We use later-treated victims as control groups under an event study framework. Among all victims who eventually report IPV to the officials, we find (i) women's employment rate decreases after the onset of violence, but an incremental positive effect follows after reporting, particularly among young and educated women; (ii) the probability of divorce increases for both men

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and women after the report but not at the violence onset; (iii) reduction in additional childbirth after reporting regardless of the victim's gender; (iv) rising depression outpatient-visits after violence and reporting among women.

Key Words: Intimate Partner Violence, Domestic Violence, Dynamic Treatment Effect

Highlights:

- The employment rate of women is reduced by 1.8% two years after IPV happens, while men are affected less in general. Young and educated women, in particular, gradually return to the labor market after reporting to the officials. The effect persists for at least 4 years.
- Men and women do not immediately file for divorce when facing IPV; more than 14 ppt of them do so after reporting the violence through official channels. The effect is homogeneous across groups.
- Regardless of gender, IPV leads to a decrease in additional childbirth by at least 3 ppt among young victims per year. The effect is persistent for at least 4 years.
- Compared to men, women tend to visit outpatient clinics more frequently for depression due to IPV.

1 Introduction

Intimate partner violence (IPV) has been a pervasive issue with a substantial impact on both male and female populations across the globe. According to World Health Organization (2021), more than one-quarter of women worldwide are subjected to IPV during their lifetime. In parallel, there has been a growing acknowledgement of the occurrence of IPV among men in recent years (Reid et al., 2008; Randle and Graham, 2011; Kolbe and Büttner, 2020). Over the past two decades, the emergence of both individual and aggregate-level data has enabled economists to delve into the causal relationship between IPV and different causes. These include socioeconomic status, cultural norms, gender ratio, and emotion-related impulsivity, among others (Aizer, 2010; Card and Dahl, 2011; Bobonis et al., 2013; Tur-Prats, 2021). However, partially attributable to data constraints, our understanding of the costs associated with IPV on victims remains incomplete. In particular, sample size, limited information on outcomes, and the unknown duration of IPV largely limit the scope of most previous studies.[1]

In this paper, we use novel administrative data to study the costs of IPV from onset to the ultimate reporting by examining a range of outcomes. Our analysis begins with an investigation of labor market performance, followed by an exploration of evolving family dynamics, including trends in divorce and fertility decisions. Finally, we assess the impact on health outcomes, with a particular focus on outpatient visits related to depression. A key innovation of this study lies in the distinction between two specific effects: the "violence effect" and the "report effect." The violence effect pertains to the direct and immediate impact of violence on various outcomes and is thus a more readily identifiable cost. In contrast, the report effect encompasses a broader range of factors, reflecting the collective impact of concurrent events at the time of reporting. Ultimately, we aim to answer the following three questions. First, how do victims respond when violence first happens? Second, what are the response dynamics when IPV is reported? Third, what are the mechanisms?

[1] This paper is an updated version of our earlier working paper (Chang et al., 2023) and was previously circulated with the title "When Love Hurts: The Impact of Intimate Partner Violence on Later Life Outcomes". The number of observations is slightly different from our earlier version due to administrative constraints; however, almost all estimates are virtually indistinguishable.

We leverage a granular administrative dataset that compiles all the reported IPV cases in Taiwan from 2012 to 2018, including but not limited to three primary report channels: the police, hospital, and helplines. It is then merged with administrative health and labor records, as well as household register data, to create a rich dataset that incorporates violence and report information. Notably, data on violence dynamics is feasible because local social workers are dispatched to assess the need for assistance in all reported cases of IPV, where essential information such as the onset of violence is documented. This advantageous feature enables us to provide a unique perspective, as we independently assess both the violence itself and the effects of reporting. This approach differs from the conventional literature, which often focuses solely on one aspect or the other.

In addition to data constraints, econometric challenges also curtail the scope of previous studies. Since IPV does not randomly happen across relationships, it presents a nontrivial task in disentangling causality from correlation, which often leads to omitted variable bias in causal estimation.[2] As an illustration, when estimating healthcare utilization between individuals who have experienced IPV to their counterparts, it is possible that the researchers are capturing inherent differences in healthcare utilization between the two groups of individuals rather than the causal effect of IPV. To address such concern, we adopt an event study design that uses the later-treated as the control group design to account for the nonrandom nature of IPV, which enables us to control for unobserved characteristics of IPV victims.[3]

Our results suggest that IPV disrupts multiple dimensions of victims' lives, from their economic stability to family dynamics, reproductive decisions, and mental health. It is shown that IPV leads to a decline in women's employment rates by 1.1 percentage points (or 1.8%) two years after the onset, reflecting the economic toll of violence. Yet, formal reporting of IPV appears to serve

[2] For example, Aizer (2011) shows that economically disadvantageous groups and racial minorities are more likely to encounter IPV in the US.

[3] This concern, as well as selection bias and simultaneity, was not well addressed until recent years. Across other disciplines such as psychology, sociology, and criminology, studies often center on descriptive analysis but fail to explicitly consider the above issues (Tolman and Rosen, 2001; Campbell, 2002; Jinseok and Gray, 2008; Rafiman et al., 2021). Recently, Bhuller et al. (2024) and Adams et al. (2024) are two papers that study the impact of IPV with a event-study framework.

as a critical turning point, with younger and more educated women responding particularly strongly to the opportunity to rebuild their lives. Overall, reporting is associated with a persistent increase in employment rates of roughly 0.8 percentage points (or 1.2%) over four years, suggesting that economic recovery plays a key role in fostering a sense of agency and enabling life transformations.

This pattern is mirrored in other domains. Divorce rates remain persistently high after IPV, indicating that many victims endure abusive relationships in the short term. However, following the reporting of IPV, we observe a substantial rise in divorce rates, particularly among younger and educated women. This suggests that reporting, coupled with the prospect of economic independence, empowers these women to separate from abusive partners and take decisive steps toward a new life. Fertility decisions show a similar trend, with no immediate change following IPV incidents but a significant decline after reporting, again among the same groups.

Mental health outcomes further illustrate the transformative effects of reporting IPV. Depression outpatient visits increase sharply during the year of reporting. Over time, these visits return to pre-report levels. This may suggest some stabilization as victims adapt to their new circumstances and begin to heal. By and large, we find similar patterns across male victims of IPV, with two notable differences. First, males' outcomes, particularly in the labor market, do not exhibit the same degree of recovery after reporting. Second, the increase in depression outpatient visits is considerably smaller in magnitude compared to females, reflecting potential differences in the way IPV affects men's mental health or in their likelihood to seek care.

Taken together, these findings suggest that IPV disrupts victims' lives in profound and interconnected ways, with reporting acting as a pivotal moment for recovery and transformation. While labor market recovery is particularly salient among younger and educated women, it may provide the economic security necessary for broader life changes, including leaving abusive relationships, re-evaluating family decisions, and seeking mental health support. These patterns underscore the importance of addressing IPV's multifaceted impacts to support victims—both female and male—as they rebuild their lives.

Previous literature has documented the cost of IPV in either violence effect or

report effect, but not simultaneously. For example, Bowlus and Seitz (2006) and Anderberg et al. (2023) use survey data to investigate violence effects through the lens of life cycle models. Relatedly, Adams et al. (2024) examine how the beginning of an abusive relationship compromises a variety of outcomes by exploiting administrative cohabitation data, even though they do not directly observe the onset of violence. On the other hand, Bhuller et al. (2024) and Bindler and Ketel (2022) merge police report data and administrative records to estimate report effects.

Most closely, we speak to two recent papers that respectively discuss violence and report effects of IPV using administrative records. Adams et al. (2024) approximate the onset of violence with the beginning of cohabitation and characterize how an abusive relationship compromises labor outcome. They show that violence effect (or cohabitation effect) in Finland causes women's employment rate to fall by 4% after two years of cohabitation, which is higher than our average estimate of 1.8% two years after violence happens. On the other hand, Bhuller et al. (2024) focus on the report effect in Norway and record changing home environment, mental health, and labor supply. Their estimates in marital dissolution and mental health are remarkably similar to our findings, while labor outcomes suggest completely opposite patterns.

Building upon the above two papers, we shed light on 4 additional perspectives. First, we distinguish the violence effect from the report effect by separately estimating them across multiple outcomes. Second, our research also documents the IPV costs among men, providing new empirical evidence and raising awareness of this issue that has been largely overlooked despite its severity in recent years. Third, we include other major report channels such as hospitals and helplines in addition to police reports. This is crucial as the factors influencing individuals' choices of reporting channels remain unclear; fortunately, our comprehensive dataset, comprising reports from various channels, enables us to make insightful comparisons. Finally, our study contributes novel evidence of the diverse costs associated with IPV in an Asian country, which differs considerably from the Western context. This is of particular interest as our findings suggest that regions with different gender in culture and social welfare systems, such as Taiwan, may require different policy approaches compared to Western countries in gen-

eral. Such a comparative analysis is imperative as IPV remains a significant issue worldwide.

From a policy standpoint, it is essential to grasp the repercussions of violence to foster a culture of help-seeking, especially because IPV is frequently endured in silence and often goes unnoticed in its initial stages. Additionally, comprehending the impact of reporting is crucial as the government seeks to determine when and how to provide assistance, such as legal protections, shelters, financial aid, and other support mechanisms.

We identify two significant policy implications. First, the observed improvements in outcomes related to mental health, employment prospects, and decisions surrounding divorce and fertility—particularly among young, educated women—underscore that economically empowered women with greater external opportunities are better positioned to recover after experiencing and reporting IPV.[4] Given the constraints on governmental resources for assistance programs, it is imperative to prioritize support for older and less-educated women, who may be particularly vulnerable. Second, the distinct patterns we identify among men, such as the frequency of outpatient visits for depression, raise concerns about the need for tailored policies that address male reporting and support mechanisms.

The paper is organized as follows. In Section 2, we first describe the background of IPV in Taiwan and around the world. Section 3 introduces data and summary statistics. In Section 4, we justify our research design. Section 5 presents the main results. In Section 6, we discuss heterogeneity and reporting behavior. Section 7 shows that our results are robust under various specifications. Section 8 concludes with potential policy implications.

[4] This finding aligns with the work of Hidrobo and Fernald (2013) and Sanin (2023), who demonstrate that economic empowerment and resourcefulness among women lead to a significant reduction in IPV. Additionally, Hidrobo et al. (2016) highlight that education further amplifies the effects of economic empowerment.

2 Background

2.1 IPV in Taiwan and around the globe

IPV has garnered recognition and international attention from the United Nations since 1996, yet it still remains one of the most ubiquitous crimes around the globe. According to the Violence Against Women Database by OECD (2023), the lifetime prevalence rate of IPV generally surpasses 30% in developing countries.[5] Surprisingly, even in OECD member countries known for their robust social welfare systems and gender equality, a significant prevalence rate of 20-30% remains prevalent. This is notably observable even in well-regarded Nordic nations, where the prevalence rate hovers around 20%.

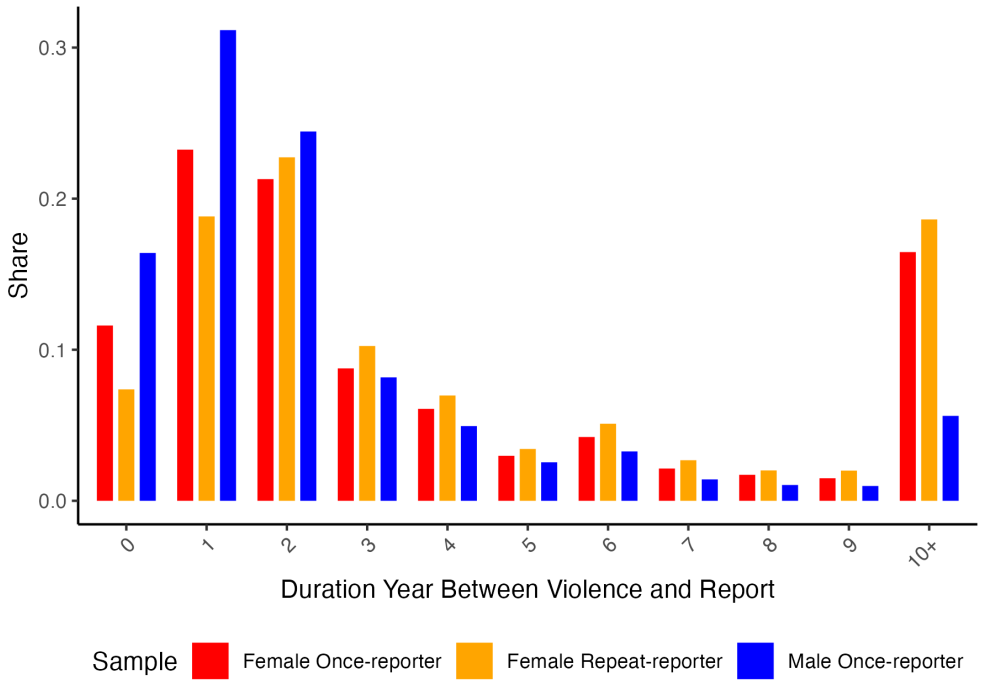
In the context of Asian societies, which exhibit differences in prevailing norms and welfare systems compared to Europe, Taiwan exhibits similarities to the aforementioned countries in terms of lifetime prevalence rates. Notably, Taiwan took the lead among Asian nations by criminalizing IPV as early as 1998 and has made significant advancements in safeguarding victims since then. However, the current lifetime prevalence rate of 25% in Taiwan indicates that both Taiwan and countries worldwide continue to confront significant challenges in addressing this issue.

2.2 Reporting IPV in Taiwan

2.2.1 From Violence to Reporting

Frequently, individuals who experience IPV exhibit a tendency to endure abusive behaviors or even extend forgiveness to the perpetrator (Li et al., 2020; García-Díaz et al., 2020). Figure 1 illustrates that, irrespective of gender, victims do not immediately seek official channels at the onset of the violence. In fact, there is a tendency of tolerance with an average delay of 2.56 years for females and 2.36 years for males; only 11.6% of females and 16.4% of males reported incidents of violence immediately.

[5] This suggests that 3 out of 10 women in developing countries have experienced IPV at some point in their lives.



Notes: This figure compares the duration year between violence onset and report among IPV reporters. Duration equal to 0 means instant reporting when violence happens. "Once-reporter" refers to individuals reporting once from 2012 to 2018, whereas "Repeat-reporter" denotes those who report multiple times.

Figure 1: Distribution of IPV Duration Among All Reporters

2.2.2 What Happens When Victims Report?

In cases of IPV reports, nearly all victims choose to report their experiences through three official channels: approximately 50% report to the police, 40% seek help from hospitals, and 10% reach out to helplines. Once a report is made, a local Domestic Violence and Sexual Assault Prevention Center (DVPC) is legally obligated to digitize and compile all the reported cases that are not obviously false; social workers are then assigned to evaluate each case individually, conducting surveys to assess the specific needs of the victims. If the severity of the cases meets certain criteria, the victims are officially classified as "severe" and provided with necessary assistance, including but not limited to access to shelters, guidance on divorce procedures, assistance with applying for protection orders, and access

to therapists.[6] This approach is reminiscent of governments with well-developed social welfare systems, such as the Norwegian government, which offers comparable programs not only for the victims themselves but also for their children.

After receiving reports, local DVPC provides several comprehensive services. First, legal expertise, including guidance on divorce decisions, is among the most commonly sought-after services. More often than not, victims are directed to apply for protection orders through the court system. Second, under specific circumstances, victims have the option to apply for financial assistance, child services, or alternative living arrangements such as temporary shelters. Third, local DVPC are equipped to guide victims toward medical treatments, including referrals to outpatient clinics or other therapists.

2.2.3 IPV-related Divorces in Taiwan

One of the outcomes that we focus on is divorce. In Taiwan, divorce decisions are based on mutual agreement and the process often takes at least several months. Couples bring their case to the court and present their arguments to the judge when a divorce agreement cannot be reached. In cases related to IPV, a judge's decision is typically influenced by the evidence provided by both parties, highlighting the importance of victims preserving evidence that supports their claims. For instance, when victims report to a local DVPC, it facilitates the process of obtaining protection orders, which can favorably impact court decisions. Another example is when victims are directed to seek medical evidence from clinics, as it serves as solid proof in court proceedings. We discuss how this affects our interpretations of the results later.

3 Data

3.1 Datasets

We leverage a granular dataset of compiled IPV reports and merge it with administrative health and labor records in order to investigate individual outcomes.

[6] Most of the time, there will be assistance provided if the TIPVDA score is 8 or higher; occasionally exceptions are made if 7 or lower. Victims' willingness also matters significantly.

We also combine two other separate datasets in order to acquire demographic information. These datasets are all linked through a unique individual identifier.

3.1.1 Domestic Violence Report File

Domestic Violence Report File (DVRF) is a compiled dataset that collects all the reported domestic violence cases in Taiwan from 2012 to 2018. Broadly speaking, domestic violence also encompasses child abuse, elder abuse, and violence among other family members; however, since this paper only explores violence among intimate partners, our main sample consists of only IPV cases from 2012 to 2018 in Taiwan with more than 10,000 cases per year. As mentioned earlier, social workers are legally required to evaluate all the reported cases, which consequently allows us to construct a rich dataset that not only includes the basic report details such as time and demographics but also a variety of questions surveyed by social workers. Most importantly, the question "How long has the violence lasted?" allows us to innovatively incorporate information on violence onset in a report dataset.

The inclusion of all report channels in the DVRF dataset enables investigation into heterogeneity arising from different reporting channels and addresses the selection bias issue that was present in previous literature due to selective samples. Previous studies, such as Bindler and Ketel (2022) and Bhuller et al. (2024) focused on police reports to examine the costs of IPV. Aizer (2011) identified cases of IPV from hospital data to study its impact on newborns. In this paper, we compile data from all report channels and will discuss the variations in outcome dynamics across these sources in later sections.

3.1.2 National Health Insurance

The Taiwanese government has implemented a comprehensive universal health care system known as the National Health Insurance (NHI) since 1995, which has facilitated the establishment of a well-preserved and digitized database encompassing population-level data comprising diverse medical records and demographic information. Our analysis focuses on the demographic module spanning the period from 2003 to 2018, which incorporates variables such as year of birth and occupation, and is sufficiently lengthy to allow examination on pre-violence be-

haviors. Notably, this dataset also includes employment variables since insurance fees are entirely contingent upon an individual's employment status and occupation type; as a result, we can effectively discern an individual's employment status using this variable alone without the need for merging external datasets.

Furthermore, we leverage the outpatient module which records all outpatient-visits spanning the interval from 2003 to 2018. Of particular interest to our study is the identification of depression outpatient-visits, which we define based on the presence of a specific battery of ICD9 codes within an individual's outpatient record. By incorporating this criterion, we can accurately identify instances of depression outpatient-visits throughout the specified time period.[7] Both modules, originally organized on a monthly basis, are transformed into yearly levels to facilitate the construction of a 16-year panel dataset for all the victims.

3.1.3 Household Register Data and Newborn Birth Record

In conjunction with the aforementioned NHI modules, we obtain valuable demographic data from the Household Register Data spanning the years 2003 to 2018. This dataset provides essential information on individual education levels and marital status. Additionally, we leverage the Newborn Birth Record dataset covering the period from 2000 to 2018. This dataset allows us to ascertain the count of newborns and the approximation of fertility decisions.

3.2 Measuring the IPV Onset

3.2.1 How Do We Identify IPV Onset?

As previously mentioned, our data regarding IPV duration before reporting is sourced from surveys conducted by social workers. Such data is feasible because, when victims report IPV through official channels, social workers are assigned to assess individual circumstances and conduct comprehensive surveys, enabling us to gather information on IPV duration and subsequently estimate the violence effects. However, it's essential to note that not all victims are subjected to these surveys; on average, less than 70% are surveyed by social workers. Possible

[7] A depression outpatient-visit is designated when any of the following ICD9 codes are identified per visit: 296, 296.0-296.8, 300.4, 309, 309.0-309.1, 311, or 311.0-311.5.

contributing factors to failed evaluation include the possibility of false reports, victims' reluctance to receive assistance, or their becoming untraceable, etc. To alleviate this concern, we perform a robustness check in Section 7.6, arguing that such missing does not compromise the validity of our primary conclusions significantly.

3.2.2 Measurement Error

Given that our entire estimation of the violence effect is based on the usage of a survey question administered by social workers, it is essential to account for the potential influence of recall bias in these reports. There are two potential sources of bias in the data. The first is the presence of bunching. In the raw data, there appears to be clustering at intervals such as 6 months, 12 months, 18 months, and so on. However, since our analysis is conducted at the annual level, this form of bunching does not affect our estimates. The second source of bias relates to recall accuracy. Due to the subjective nature of IPV, verifying self-reported information poses significant challenges. To address this, we compare our data to a 2017 government report (Pan et al., 2017), which conducted direct interviews with recruited individuals, in contrast to our administrative data where the sample consists of IPV reporters. Despite the limited sample size, the distribution of IPV duration reported by these women closely mirrors the findings in our study.

Furthermore, we argue there is little incentive for individuals to misreport the duration of IPV, as this factor has a limited impact on social workers' assessments regarding the provision of support. Instead, social workers base their evaluations on a range of variables related to the severity of the situation and the safety of the individuals involved.

3.3 Summary Statistics

By integrating the datasets discussed earlier, we provide summary statistics in this section for all reported cases, categorized into three distinct subgroups: female once-reporters, female repeat-reporters, and male once-reporters.[8] "Once-reporters" are defined as individuals who reported only once during the period

[8] Male repeat-reporters are excluded from the analysis due to their small sample size.

Table 1: Summary Statistics by Number of Reports of IPV in 2012-2018

Sample: All 25 to 55-year-old	All Female Population			All Male Population	
	Non-victim /Non-reporter	Once-reporter	Repeat-reporter	Non-victim /Non-reporter	Once-reporter
Panel A: Victim Demographics					
Mean Age	40.30	39.61	37.96	40.30	40.48
Mean Education Years	13.19	12.27	11.89	13.23	13.04
Depression Outpatient-visit	3.3%	9.6%	12.8%	2.0%	4.9%
Panel B: Social Worker Evaluation					
Labeled "Severe" & Provided Assistance	-	33.1%	40.6%	-	19.4%
IPV Severity Index (Out of 13)	-	3.66	4.03	-	3.04
Panel C: Perpetrator Statistics					
Mean Age	-	42.84	41.71	-	38.4
Mean Education Years	-	11.72	11.21	-	12.21
Max Wage In 10 Years Before Report	-	375,601	334,892	-	293,392
Panel D: Family Demographics					
With Child Under 12	31.1%	49.2%	56.4%	32.8%	47.8%
If Yes, How Many?	1.51	1.63	1.76	1.54	1.53
Unmarried	28.3%	10.6%	10.1%	35.9%	19.5%
Married	59.3%	69.9%	69.3%	54.6%	65.0%
Divorced	10.6%	18.6%	19.3%	9.2%	15.2%
Panel E: Employment Status					
With Stable Employer	60.0%	45.0%	37.7%	61.6%	59.2%
Public Worker	5.5%	2.3%	1.7%	4.9%	4.8%
Generic Private Worker	54.5%	42.7%	36.0%	56.7%	54.4%
Mean Wage	35,054	29,724	28,022	40,473	40,312
Max Wage In 10 Years Before Report	-	311,838	268,754	-	371,999
Panel F: Report Channel					
Police	-	49.4%	49.4%	-	52.7%
Hospital	-	39.0%	37.7%	-	39.8%
Helpline	-	7.0%	7.9%	-	4.3%
Other	-	4.6%	5.0%	-	3.2%
Panel G: IPV Duration					
Survey Rate	-	63.0%	44.7%	-	39.1%
If Surveyed, Duration = 0	-	11.6%	7.3%	-	16.4%
If Surveyed, Duration >= 10	-	16.5%	18.6%	-	5.6%
If Surveyed, Duration > 0 & < 10	-	71.9%	74.1%	-	78.0%
Mean Duration If Duration > 0 & < 10	-	2.56	3.02	-	2.36
Observations	5,679,083	90,604	25,753	5,558,734	9,150

Notes: For both once-reporters and repeat-reporters, all statistics are calculated with data 12 months prior to IPV reports from DVRF and NHI datasets; for non-victim/non-reporter, we include all the population from age 25 to 55 in 2014 and calculate all statistics solely from NHI dataset. Male repeat-reporters are excluded from the analysis due to small sample size. All wages are measured in NTD per year.

from 2012 to 2018, while "repeat-reporters" are those who reported multiple times

within this timeframe.[9]

We focus on individuals aged 25 to 55 and analyze the demographic variables and employment status across various groups. Among women, the data reveals that those who report multiple times are generally younger, less educated, with more children under 12, and have unstable employment with lower earnings compared to once-reporters. These findings align with the observations made when comparing reporters to non-victims/non-reporters. Among men, similar conclusions can be drawn when comparing once-reporters to non-victims/non-reporters, although the pattern is not as pronounced overall. In summary, our findings indicate that economically disadvantaged women face a higher risk of experiencing IPV, which is consistent with the findings of Aizer (2011) in the U.S., where individuals with low SES are at a heightened risk of becoming IPV victims.

A similar pattern emerges in the case severity categories presented in Panel B. Case severity for female once-reporters tends to be lower compared to those who report repeatedly. Notably, the average case severity for females is higher than that for males, potentially indicating variations in reporting behavior or underlying relationship dynamics. In Panel C, perpetrators associated with female once-reporters are observed to be older, more educated, and exhibit higher employment prospects compared to those associated with female repeat-reporters. Additionally, pronounced gender differences are evident, with male perpetrators being older, less educated, and earning higher incomes in the labor market.

With respect to reporting behaviors, while Panel F shows no significant differences in channels across all three groups, more details on reporting behaviors will be elaborated in Sections 6.3. In terms of report timing, it is shown in Panel G that female repeat-reporters exhibit a tendency to endure IPV for extended periods before initiating their first report. A comparison between once-reporters reveals a discernible gender difference where males delay their reporting by an average of 2.36 years, compared to 2.56 years for females.

[9] Given the limitations of the data, we are unable to determine whether once-reporters had submitted reports prior to 2012. To account for this uncertainty, we conduct a robustness check, detailed in Section 7.7, where once-reporters are redefined as those who reported only once between 2014 and 2018, with no reports between 2012 and 2013. The primary findings remain robust to this alternative definition.

4 Empirical Strategy

To causally characterize how IPV influences individual outcomes, we closely follow Callaway and Sant'Anna (2021)'s event study framework and use individuals who also experience the event but at a later time (later-treated) as the control group to study the dynamic outcomes. We adopt the later-treated as the control group in our main specification because individuals who also experience the event are arguably more similar to the treatment group than those who never experience the event (never-treated). The approach of using later-treated individuals is also employed in Bhuller et al. (2024), while the alternative approach of using never-treated individuals is found in Adams et al. (2024).

4.1 Empirical Design

Under Callaway and Sant'Anna (2021)'s framework, we estimate the dynamic average treatment effect (ATT) for each cohort and event time, and then calculate the mean of these dynamic ATTs weighted by cohort sizes.

Formally, we define cohort g as individuals experiencing the event in year g, and r as the relative event year. For example, when studying the effect of violence, cohort $g = 2010$ includes individuals who experience violence in 2010; $r = 1$ for the 2010 cohort corresponds to the time $g + r = 2010 + 1 = 2011$.

The estimation is performed separately for each cohort and event time pair (a pair of (g, r)). For each cohort g at relative time r, we construct a subsample and estimate a simple difference-in-differences specification. Let r_0 denote the baseline year. The subsample for cohort g at relative time r includes the treatment group (individuals who experience the event in year g) and the control group (individuals who experience the event later than $g + r$) at times $g + r$ and $g + r_0$. We then estimate the following regression:

$$Y_{it}^g = \alpha_i^g + \theta^{g,r} \mathbb{1}\{t = g + r\} + \gamma^{g,r} D_i^g \mathbb{1}\{t = g + r\} + \epsilon_{it}^g, \tag{1}$$

where Y_{it}^g is the outcome variable for individual i in time t, α_i^g denotes individual fixed effect,[10] D_i^g is a dummy variable equal to 1 for the treatment group (cohort g, i.e., early reporters, reporting in year g), and $\mathbb{1}\{t = g + r\}$ is an indicator

[10] We allow α_i^g to differ across subsamples, so we add a superscript g to clarify.

function equal to 1 if the calendar time t is $g + r$. $\gamma^{g,r}$ represents the ATT for cohort g at event time r. After estimating $\gamma^{g,r}$ for each cohort, we take a weighted average based on cohort size to obtain the dynamic ATT.

To enhance comparability between the treatment and control groups in this procedure, we also conduct an inverse probability weighting (IPW) procedure as proposed in Callaway and Sant'Anna (2021). The IPW procedure reweights the later-treated controls to match the treated families on the distribution of observable characteristics. Specifically, these observable characteristics include the victim's level of education and age when we estimate the propensity score for IPW.

In addition to our main specification, we perform a series of alternative specifications to demonstrate robustness check in Section 7. These include variations in the variables used for IPW, estimation using never-treated individuals as comparisons, alternative event study specifications such as canonical two-way fixed effects estimation, the event study methods in Sun and Abraham (2021), and the decomposition approach from Goodman-Bacon (2021), monthly level analysis, and alternative levels of standard error clustering.

4.2 Sample Construction

Our primary analysis focuses on female once-reporters, with the cases of female repeat-reporters and male once-reporters examined separately in Section 6. As an initial step, we exclude individuals identified as both victims and perpetrators from the sample. This results in the removal of 7.9%, 26.8%, and 62.9% of female once-reporters, female repeat-reporters, and male once-reporters, respectively, from the original dataset.[11] Subsequently, when estimating the report

[11] We exclude 62.9% male victims who are also perpetrators from our analysis and the remaining sample consists of 9,150 individuals. The phenomenon of individuals simultaneously engaging in both victim and perpetrator roles, often referred to as "mutual violence" or "reciprocal violence", has been extensively explored in the literature. Notable studies, such as Moffitt et al. (2000) and Langhinrichsen-Rohling (2010), examine the prevalence of this pattern, while Whitaker et al. (2007) specifically highlight that men are more likely than women to occupy both roles. Several factors contribute to this phenomenon, including shared exposure to adverse environmental conditions and instances where partners retaliate by reporting each other to authorities. While this dynamic presents an important area for investigation, it falls outside the scope of the current paper and will be left for future research.

effect, our main sample comprises 90,604 female once-reporters.

To estimate the violence effects, we focus on those who were surveyed and answered the question "How long has the violence lasted", which drops more than 30% of the sample. We then remove individuals who report violence more than 10 years later in order to mitigate potential recall bias and measurement error; individuals reporting IPV immediately were also removed and separately discussed in Section 6.3.2 due to ambiguous definitions of violence and report effects.[12] This process results in a remaining sample of 41,057, which is essentially all female once-reporters whose IPV duration falls within 1 to 9 years.

We plot the raw data for female once-reporters and a matched sample of never-reports with similar year of birth and level of education in Figure A.1. We find substantial differences between female once-reporters and their matched counterparts, reinforcing the notion that IPV occurrence is non-random.

5 Results

We begin by presenting both violence and report effects among female once-reporters of our main outcomes in the following sequence - labor market performance, divorce decisions, fertility decisions, and health outcomes. Labor outcomes are discussed first as it's considered a critical metric for informing policy; stable employment is closely linked to women's economic independence, which in turn increases their capacity to escape cycles of violence. Achieving financial stability through secure employment is fundamental to empowering women, enhancing their autonomy, and reducing their exposure to violence. Subsequently, findings on divorce and fertility decisions are presented to offer a complementary perspective. Finally, we extend the analysis to encompass an additional critical dimension of welfare: health outcomes, with particular emphasis on mental health approximated by the frequency of depression outpatient visits.

Caution is necessary when interpreting our findings. First, we encounter the same limitation found in other studies utilizing report data as only part of the IPV victims choose to disclose their experiences. Therefore, both violence and re-

[12] According to Figure 1, we roughly lose another 25% of the sample for the two removal combined.

port effects under our framework should be considered as the ATT for those who eventually report the incidents within the observed years. In a study by Pan et al. (2017), it was estimated that the lifetime prevalence rate of IPV among women in Taiwan is 25%, yet only 3% out of the 25% reported through official channels. It is likely that these reported cases are more severe in nature, as the individuals involved made the conscious decision to come forward.[13] Second, causal interpretation of the report effect is tricky due to concurrent events and unobserved changes in family dynamics. Specifically, if the act of reporting exacerbates the abusive behavior of the husband or is perceived as a signal that the woman has relinquished her efforts to salvage the toxic relationship, causal estimates of report effects should be interpreted carefully.

5.1 Labor Market Performance

Due to data constraints, we decide to focus on the more stringent definition of labor supply - the probability of having a full-time job. We define the outcome variable, an annual work dummy, as taking the value of 1 if the individual works more than six months in a given year, and 0 otherwise.[14] In our analysis, this is broadly considered the "annual employment rate" in the event-study style figures.[15]

In Panel A of Figure 2, the violence effect indicates an adverse IPV impact on the employment rate across all post-periods. While the initial impact is relatively modest, we observe an immediate decrease of 0.6 ppt a year after the onset, followed by another 1.1 ppt (or 1.8%) decrease in the subsequent year. The dynamic DiD coefficients, albeit bouncing back slightly two years after the onset,

[13] Collecting data on the frequency of all incidents of IPV at the population level, though technically feasible, is a challenging task. This is because IPV often goes unobserved, and conducting a survey asking women about their IPV experiences would require significant manpower and resources. Additionally, merging this sensitive dataset with administrative records is difficult due to privacy concerns.

[14] We choose this definition because NHI data does not identify part-time jobs at all; as to other labor market outcomes such as working months per year or annual income, we discuss later in this section and find similar patterns.

[15] Although data of employment rate and health outcomes exist in our data, we prefer the year-level analysis to maintain consistency with other outcomes, such as divorce and fertility decisions, which are measured annually due to data constraints. We provide more discussion in Section 7.4.

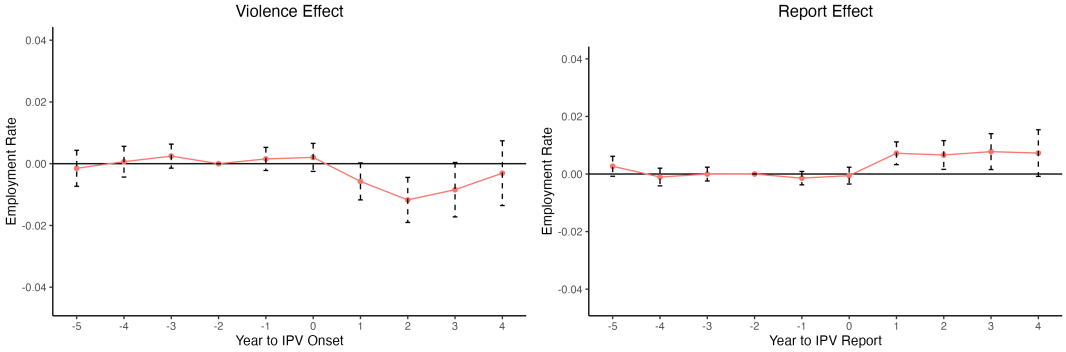
consistently show negative signs, highlighting the distressing nature of IPV and its adverse effects on the employment rate. It is worth noting that the reversal in year 2 reflects a weighted average effect among people who report 1 to 9 years after the onset of violence. The trend continues to rise as women gradually return to the labor market; at the end of the fourth year, the adverse outcome is almost non-existent. The report effect echoes this observation by showing that women consistently reintegrate into the labor market following their outreach and reporting of violence. This positive effect persists, reaching almost 0.8 ppt 4 years after the reporting event, which translates to a 1.2% increase compared to the period -2.

Interestingly, we observe mirroring patterns in the figures. The inverse pattern in the violence effect suggests a positive report effect, given that reporting can occur at any point after the onset of IPV. Conversely, in the report effect, the slight dip beginning in period -1 could be attributed to a negative violence effect, as IPV can occur at any time before the report. Combining the insights from both figures, we conclude that while women's employment rate slightly improves after reporting, the overall employment effect remains negative when compared to the pre-IPV employment rate.

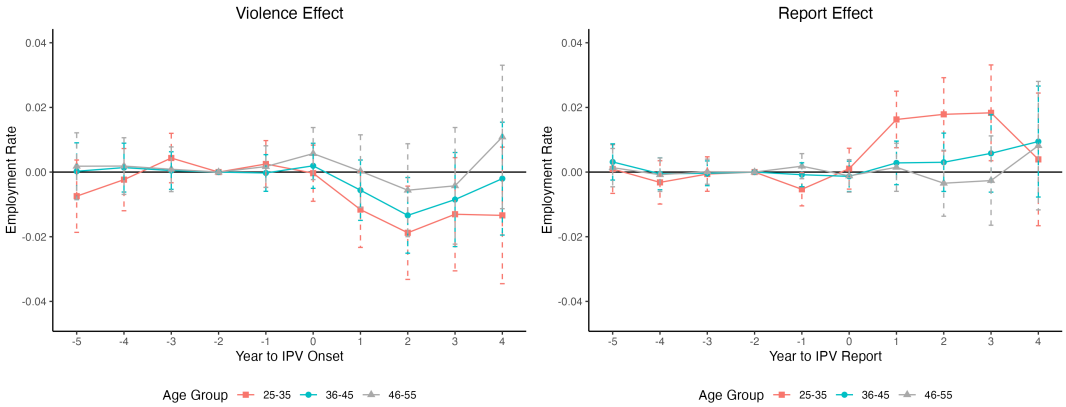
In Figure 2 Panel B, we further analyze the effects by age group at the time of reporting. The violence effect indicates that both the youngest cohort (ages 25-35) and the middle-aged group (ages 35-45) experience an overall decline in employment rates; however, only the youngest cohort demonstrates the ability to re-enter the labor market. When focusing on this group and disaggregating the sample by education level in Panel C, the violence effect reveals that individuals without graduating from high school suffer slightly more, whereas those with at least a high school diploma show the capacity to re-enter the labor market. We present the average effects in Table B.2 and Table B.3.

To contextualize our report effect results, we first compare them to the findings of Bhuller et al. (2024) in the Norwegian setting. They highlight negative impacts following the act of reporting under a similar event study framework. Furthermore, while our study represents the first to examine the violence effect separately from the report effect, Adams et al. (2024) also provides some insight when estimating the cohabitation effect of IPV in Finland. Their research indi-

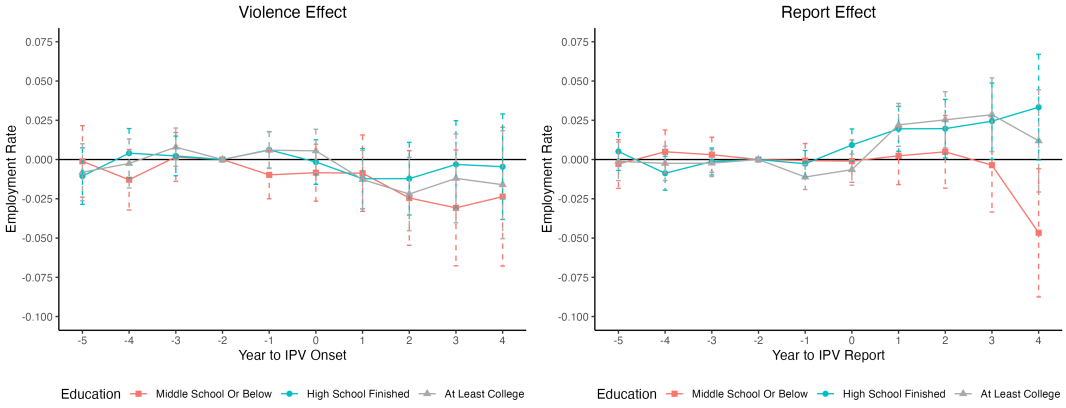
Panel A: Overall Effect



Panel B: Reporting Age Breakdown



Panel C: Education Breakdown Among Age 25-35



Notes: Panel A presents dynamic event-study coefficients for both violence and report effects, which are then decomposed into 3 age groups in Panel B; Panel C further builds on the young and is broken down by education level. Dash lines are 95% confidence interval of the estimates. The outcome variable, *Employment Rate*, is defined as 1 if the victim works more than 6 months in a given year, and 0 otherwise.

Figure 2: IPV Effects on Employment Rate

cates that cohabitation causes a 4% decline in women's employment rates after two years of cohabitation, which surpasses our estimate of a 1.8% decline two years after the occurrence of violence.

Four reasons plausibly explain such discrepancy. First, significant disparities exist in work incentives and welfare systems between Taiwan and Nordic countries. For instance, expectations of more generous unemployment insurance and various non-financial support mechanisms in Nordic nations can influence individuals' attitudes towards returning to work after reporting, setting the stage for variation between the two regions. Second, characteristics among those who choose to report incidents may inherently differ due to distinct expectations of the support available and broader reporting customs. Third, both Bhuller et al. (2024) and Adams et al. (2024) leverage only police data, while our data also includes hospital and helpline data.[16] Lastly, data limitations could play a role. As the NHI in Taiwan does not disclose information about part-time employment, there is a possibility that we underestimate the impact on the labor market by not accounting for improvements in part-time employment status.

In addition to analyzing the employment rate, we examine two additional labor market outcomes: the number of working months per year and annual income. In Figure C.1, Panel A, we replace the annual work dummy, previously coded as 1 if an individual worked at least six months in the year, with the actual number of working months as the dependent variable. The results reveal a broadly consistent pattern across both the effects of violence and reporting. In Panel B, we analyze annual income as the outcome variable and observe a similar pattern to that of the employment rate. However, unlike the employment rate, annual income displays a progressive trend over time, which may be attributable to life-cycle effects.

More often than not, employment status and other labor outcomes can be endogenous and lead to simultaneity issues (Macmillan and Gartner, 1999; Anderberg et al., 2016; Bhalotra et al., 2021). In other words, if unemployment or decline in income is the main driver of IPV or any reporting, we shouldn't expect any discontinuous jump around period 0 either during IPV or when the victims report; in addition, we ought to observe the presence of pre-trends as they effectively lead to violence. Reassuringly, all of our labor outcomes not only

[16] We discuss report behavior in Section 6.3.

demonstrate the lack of pre-trends but also indicate the discontinuous jumps of the violence effect and report effect respectively.

5.2 Family Formation Outcomes – Divorce

It is shown that female once-reporters generally experience adverse violence effects on labor market performance, with re-entry into the labor market being limited to younger and more educated individuals. We now turn to an examination of divorce decisions, exploring whether similar patterns emerge and whether these decisions are linked to labor market outcomes.

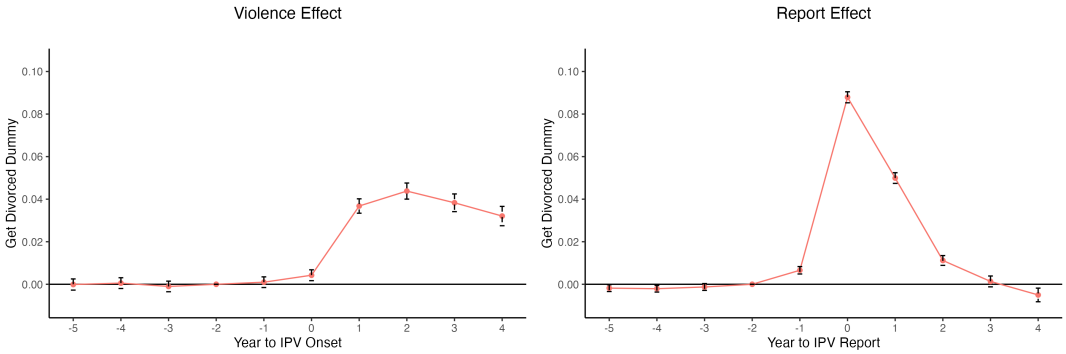
The outcome variable, get-divorced dummy, is defined as 1 if the victim gets divorced in a given year, and 0 otherwise. Figure 3 Panel A illustrates the effect of violence, showing that only 0.4 ppt of women file for divorce in the year that IPV occurs. However, a sustained increase in divorce rates exceeding 3 ppt is observed in the subsequent years. Notably, IPV reports may occur at any point beginning one year after the violent incident, highlighting the need to consider the influence of reporting when interpreting the effects attributed to violence itself. We posit that women often exhibit a tendency to initially tolerate violent behaviors and refrain from taking immediate marital actions when violence occurs. Instead, they delay filing for divorce until eventually reaching out through official channels.

The report effect in Figure 3 Panel A further attests to this. We note an 8.8 ppt increase in divorces during the reporting year, followed by an additional 5.0 ppt in the subsequent year.[17] According to Table B.1, the percentage of married couples among female once-reporters decreased from 68.9% to 54.5% 4 years after the report, which indicates marriage dissolution of 21%.

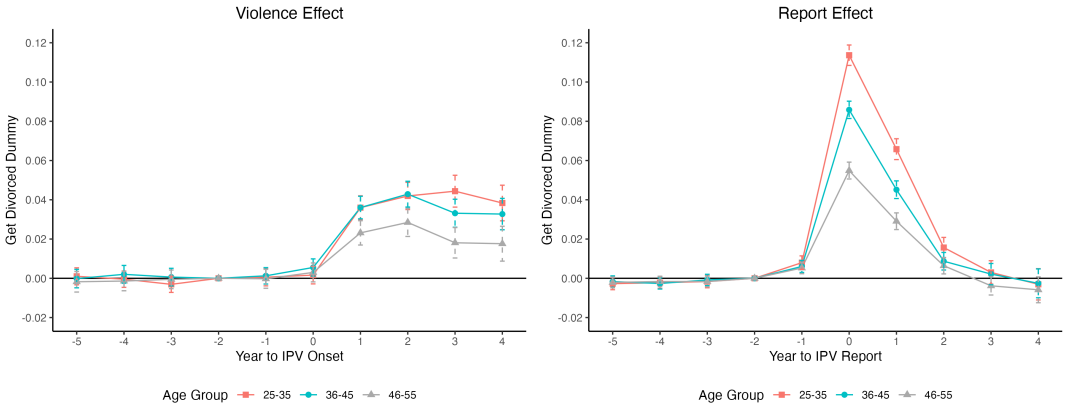
In Figure 3 Panel B, the disaggregation by age reveals that younger individuals display a greater propensity to file for divorce following incidents of IPV or the reporting thereof. When focusing specifically on the youngest cohort and further stratifying by educational attainment in Panel C, we find that people with more education only file for divorce marginally more in figures of violence and report effects.

[17] Although people can always re-enter marriage, it is mechanically impossible to get divorced two years in a row under our definition of "get divorced", as one can only get divorced if married in the previous year.

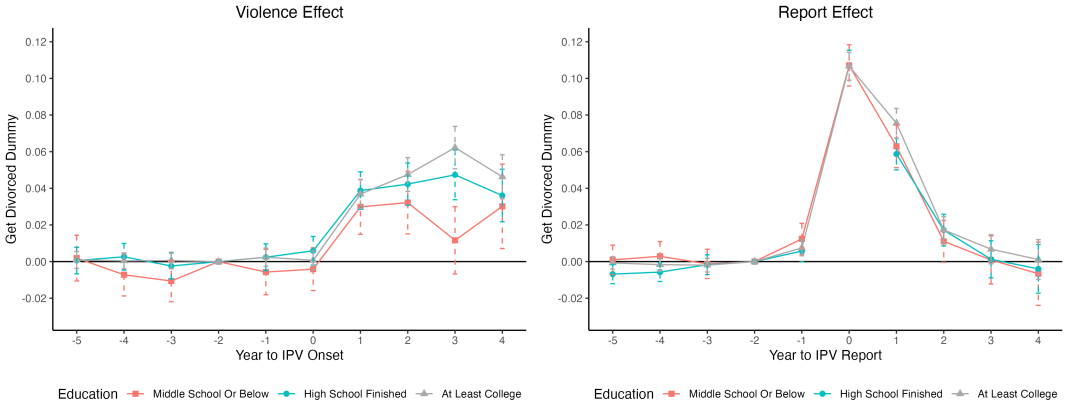
Panel A: Overall Effect



Panel B: Reporting Age Breakdown



Panel C: Education Breakdown Among Age 25-35



Notes: Panel A presents dynamic event-study coefficients for both violence and report effects, which are then decomposed into 3 age groups in Panel B; Panel C further builds on the young and is broken down by education level. Dash lines are 95% confidence interval of the estimates. The outcome variable, *Get-divorced Dummy*, is defined as 1 if the victim gets divorced in a given year, and 0 otherwise.

Figure 3: IPV Effects on Divorce Decision

We compare the background of previous studies, which predominantly focus on the West, to our context in Taiwan. By and large, tax incentives, social welfare systems, and marital culture are vastly different. For instance, the crude rate of marriage and divorce are higher in Taiwan on average while cohabitation behavior is much more common in the West.[18] We juxtapose our findings on the report effect with those of Bhuller et al. (2024) in Norway and offer two remarks. First, we observe analogous dynamic trends in both Taiwan and Norway when utilizing an event study framework. The absence of pre-existing trends suggests that, in both countries, victims do not rush into divorce decisions before reporting IPV; instead, divorce rates only significantly increase following the reporting. Second, despite the general similarity in patterns, there is a notable disparity in magnitude. Among female victims, it is documented that 50% of the marriages dissolve in Norway after reporting IPV while there are only 21% in Taiwan.

In addition to the factors mentioned earlier, there may be other unaccounted variables, including variations in help-seeking behavior, institutional differences, and the distinct characteristics of victims, that could contribute to this variation. One notable institutional factor that might contribute to the significant increase in divorce rates is the legal aspect. In Taiwan, when victims initiate divorce proceedings in court, a report record can be advantageous, serving as concrete evidence of an unhappy marriage.

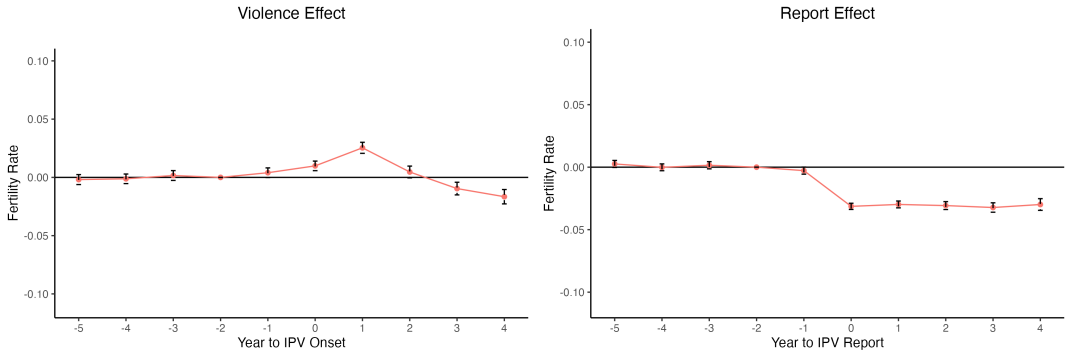
5.3 Family Formation Outcomes – Fertility

Thus far, our analysis has demonstrated that young, educated female once-reporters are more likely to experience outcomes such as securing employment or obtaining a divorce following their report. We now shift our focus to fertility decisions, investigating whether comparable patterns can be observed.

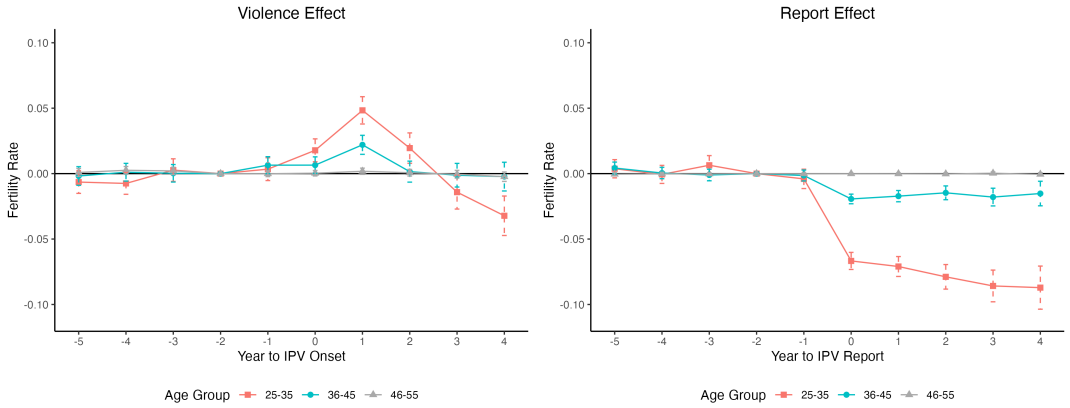
The outcome variable, give-birth dummy, is defined to be 1 if the victim gives birth to a child in a given year, and 0 otherwise. In Figure 4, Panel A presents the impact of violence over various time periods, demonstrating a subtle upward

[18] Taiwan Ministry of Interior (2021) and Eurostat (2021) have calculated the crude marriage and divorce rates to be 4.9 and 2.0, in Taiwan, and 3.9 and 1.7, in Europe for the year 2021. According to Yang (2014) and OECD Family Database (2016), cohabitation rate among individuals aged 20-34 in Taiwan was merely 4% around 2011, significantly lower than the OECD average of 17% (with Norway having the highest rate at 23%).

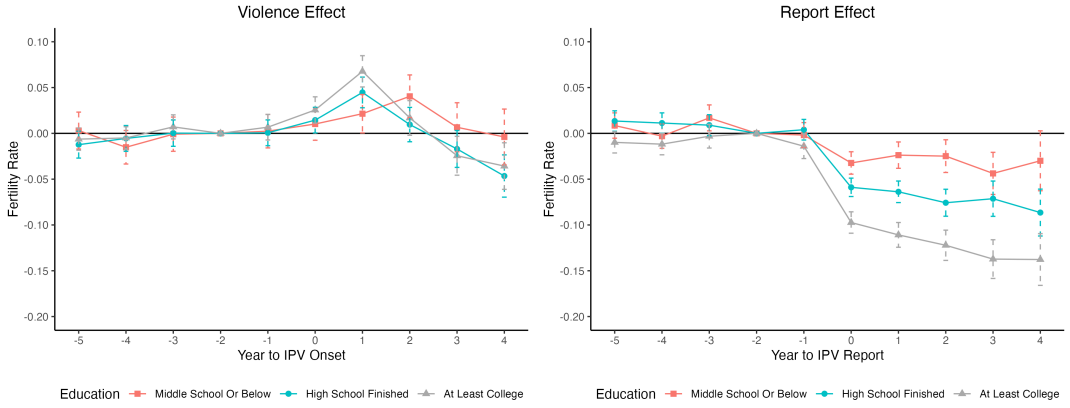
Panel A: Overall Effect



Panel B: Reporting Age Breakdown



Panel C: Education Breakdown Among Age 25-35



Notes: Panel A presents dynamic event-study coefficients for both violence and report effects, which are then decomposed into 3 age groups in Panel B; Panel C further builds on the young and is broken down by education level. Dash lines are 95% confidence interval of the estimates. The outcome variable, *Give-birth Dummy*, is defined as 1 if the victim gives birth to a child in a given year, and 0 otherwise.

Figure 4: IPV Effects on Fertility Rate

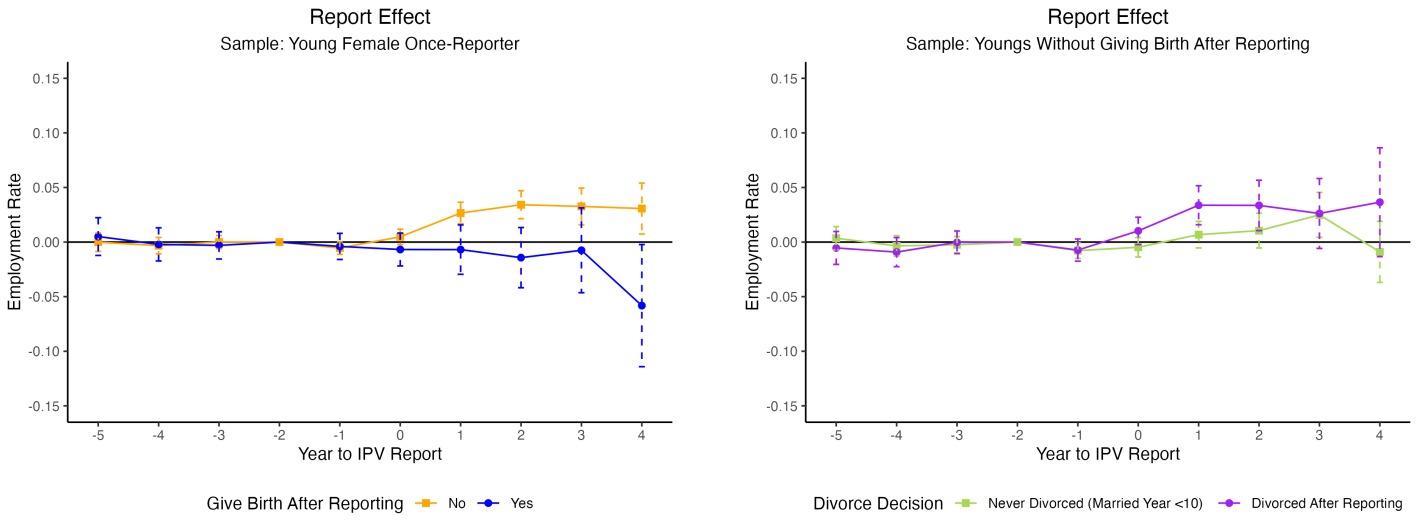
trend beginning at period -1, which may indicate a life-cycle pattern in birth-giving behavior over time. This trend is further corroborated by the results in the report effect figure, where we find a 3.0 ppt reduction in the number of additional childbirths 4 years after reporting; such magnitude is equivalent to a 47% decline in the fertility rate compared to the baseline value of 6.4% in period -2. The immediate reduction in fertility rate in period 0 suggests that violence likely occurred at least 6 to 12 months prior to reporting for most people, which is consistent with what Figure 1 suggests.

When examining the effects stratified by reporting age, Panel B in Figure 4 indicates that the impact of reporting is primarily concentrated among younger women, specifically those aged 25 to 35, consistent with expectations grounded in biological considerations. Within this demographic, Panel C further reveals that women with higher levels of education exhibit a decline in birth rates relative to pre-reporting levels. These findings align with observed patterns in labor market performance and divorce decisions, suggesting that younger and more educated individuals experience more favorable outcomes following the act of reporting. In addition to dynamic coefficients, average effects are presented in Table B.2 and Table B.3.

Anderberg et al. (2023), using life-cycle models, is one of the few papers that causally discusses the impact of IPV on the fertility rate. They leverage survey data in the UK and also find a reduced fertility rate. It is worth noting that our estimate of a consistent 3.0 ppt reduction among the victims exacerbates the issue of low fertility in Taiwan. In fact, among all countries in the world, Taiwan ranks consistently at the bottom in terms of fertility rate in the past decade according to United Nations Population Division (2019).

5.4 Interplay Between Labor Outcomes, Divorce, and Fertility Decisions

In the preceding sections, we identified consistent patterns in violence and report effects on employment rates, as well as on divorce and fertility decisions. However, these three outcomes may be interdependent, as they are potentially determined endogenously as part of a broader decision-making process. In an attempt to disentangle the three forces, we focus on report effects among young once-reporters



Notes: The left figure breaks down the report effect among the young (25-35) by fertility decision; the right figure further decomposes the orange line on the left by divorce decision. Dash lines are 95% confidence interval of the estimates.

Figure 5: Labor and Divorce Decisions Among Young Female Once-reporters

and discuss the contributing factors to rising patterns in the employment rate.

We start with those aged 25-35 in Figure 2 Panel B and further break down the report effect by fertility decision into the orange and blue lines on the left of Figure 5. This pattern reveals that the observed reentry in the labor market is predominantly driven by individuals who did not give birth following the report. Subsequently, we decompose the orange line based on divorce decisions. The resulting analysis, represented by the purple line, indicates that individuals who got divorced after reporting exhibit a higher propensity to reenter the labor market compared to those who remained married.

So far, it is shown that, in addition to age and education, decisions related to divorce and fertility may play a significant role in explaining the observed positive report effects on employment rates. However, we acknowledge that within the current framework, the interconnectedness of labor market participation, fertility, and divorce decisions poses substantial challenges to disentangling their individual effects. Our approach does not adequately address the potential for omitted variable bias, and we recognize the methodological limitations inherent in this

analysis. A more rigorous and comprehensive causal investigation is reserved for future research.

5.5 Health Outcomes

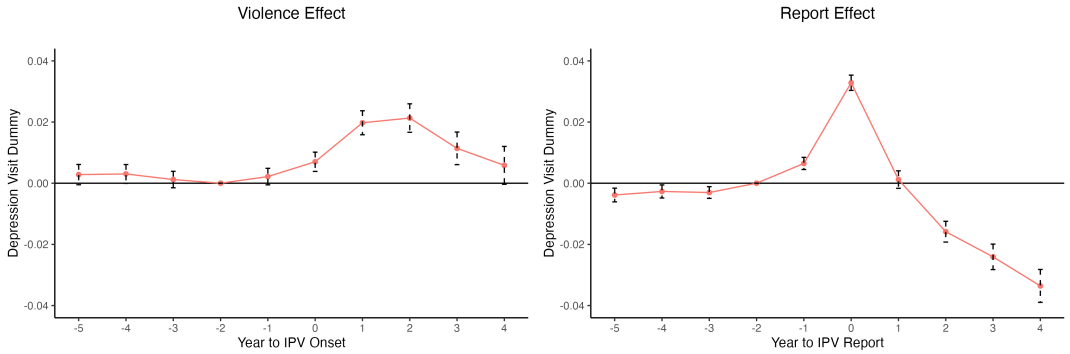
Finally, we analyze the impact of violence and report on health outcomes, with a particular emphasis on mental health. Specifically, we investigate the association between IPV and depression outpatient visits. The outcome variable, *Depression Visit Dummy*, is defined as 1 if the victim had a depression outpatient visit in a given year, and 0 otherwise. As shown in Panel A of Figure 6, the results indicate that depression outpatient visits initially exhibit a marginal increase at the onset of IPV, followed by a sustained intensification in all subsequent periods. Since the violence effect captures individuals who report between 1 and 9 years after IPV, we hypothesize a potential correlation between depression outpatient visits and reporting behavior. This hypothesis is further supported by our estimation of the report effect, which reveals a sudden increase of 3.3 ppt during the reporting period, followed by a subsequent decline. It is worth noting that our results in report effect are similar to Bhuller et al. (2024).

Panels B and C present the effects disaggregated by age and education level of the victims, using a framework similar to the previous analysis. Interestingly, the results do not reveal any consistent patterns as earlier. Individuals typically associated with greater bargaining power, such as younger or more educated victims, do not appear to exhibit distinct behavioral responses compared to their counterparts.

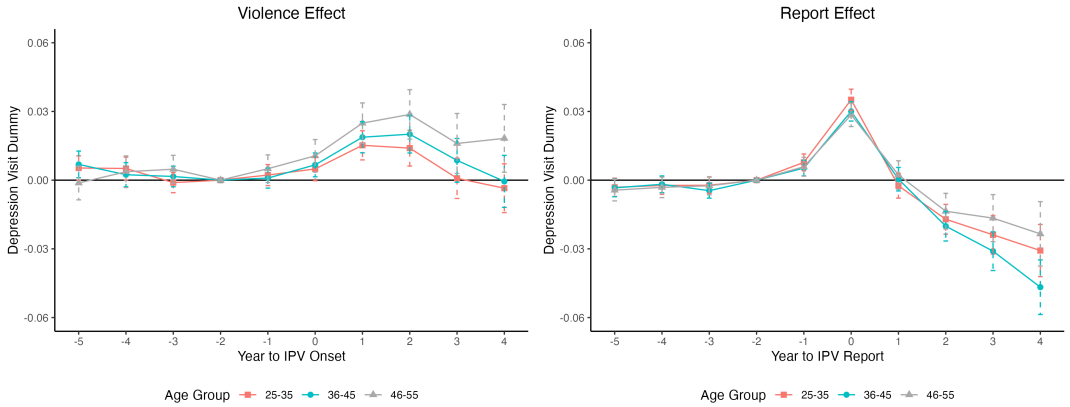
In addition to using a depression visit dummy as the outcome variable, we analyze medical expenses and show the results in Figure C.2. We observe a similar trend as in depression visits, with victims appearing to utilize greater medical resources following the reporting of incidents. Notably, the total magnitude of overall medical expenses is more than twice that of depression expenses.

We identify three potential explanations for these patterns. First, women may visit clinics more frequently due to mental health issues stemming from traumatic incidents, which likely prompted them to seek help initially. Second, visits to depression clinics might be motivated by the need to gather evidence for divorce lawsuits, though isolating this effect is empirically challenging. Third, victims may

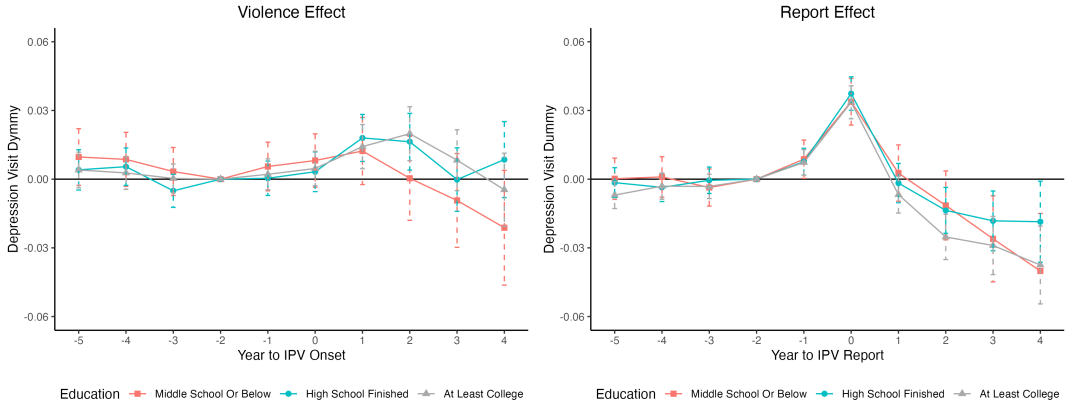
Panel A: Overall Effect



Panel B: Reporting Age Breakdown



Panel C: Education Breakdown Among Age 25-35



Notes: Panel A presents dynamic event-study coefficients for both violence and report effects, which are then decomposed into 3 age groups in Panel B; Panel C further builds on the young and is broken down by education level. Dash lines are 95% confidence interval of the estimates. The outcome variable, depression visit dummy, is defined as 1 if the victim has a depression outpatient visit in a given year, and 0 otherwise.

Figure 6: IPV Effects on Depression Outpatient Visit

receive assistance navigating healthcare resources, increasing outpatient visits. However, decomposing these effects is limited by data constraints and beyond this paper's scope.

Another limitation in our estimation is the ability to measure mental health (more discussion in Bindler et al., 2020). Since the victims can always go to a therapist or some other informal channels for help, we do not capture the full mental state of the victims throughout the course of the violence. Therefore, even though we observe a dropping pattern in the usage of depression clinics, it is possible that the victims have no incentive to visit doctors for legal proof. With all these concerns and limitations, we are reluctant to make any conclusion such as if women are mentally in a better place after reporting.

6 Heterogeneity

6.1 What About (Female) Repeat-Reporters?

Thus far, our analysis has been exclusively centered on female once-reporters. We opted for this selection for two primary reasons. First, the amalgamation of individuals who reported IPV only once with those who reported it multiple times could potentially introduce biased results, owing to unobservable factors. As indicated in Table 1, female once-reporters tend to be relatively older, possess higher educational attainment, and enjoy a more favorable socioeconomic status compared to their counterparts. Second, even if concerns about unobservable variables were set aside, defining a clear "event" for individuals reporting more than twice becomes intricate due to the possibility of overlaps in multiple reports. As a compromise, when discussing the impact of reporting or violence, we consider the first report (or the onset of violence) as the pivotal event time within an event study framework. Thus, period 0 denotes the time of the first report when analyzing the report effect and the onset of violence when investigating the violence effect. Nevertheless, it's crucial to acknowledge that ongoing IPV may persist in subsequent periods, potentially exerting influence on estimations for both violence and report effects.

In Figure A.2 and Table B.4, Panel A examines the labor market outcomes of female repeat-reporters. The analysis reveals that while IPV adversely af-

fects both once-reporters and repeat-reporters, the former group shows a greater propensity to re-enter the labor market. Regarding divorce decisions, the findings indicate that repeat-reporters not only experience delays in initiating divorce but also exhibit a higher likelihood of divorcing four years after reporting, relative to once-reporters. Panel C highlights that the fertility rate among repeat-reporters declines gradually, beginning three years after the onset of violence. Additionally, when women decide to report IPV, the reporting itself is associated with a slightly more pronounced reduction in fertility rates. Lastly, Panel D indicates minimal variation in the frequency of outpatient visits for depression across groups.

Broadly, we posit that, compared to once-reporters, repeat-reporters are more likely to remain trapped in abusive relationships following their initial report. This hypothesis is consistent with the observed delay in marital dissolution and the gradual decline in fertility rates, which becomes evident only in the third year after reporting. Further evidence of an increased tolerance for IPV is reflected in the prolonged time lapse between the onset of violence and the initial report, as shown in Figure 1. These findings underscore important policy implications, suggesting that additional resources and targeted interventions should be directed toward repeat-reporters, particularly following their second report.

6.2 Costs Among Male Victims

In recent years, IPV among males has emerged as a critical issue, garnering increasing societal recognition. Contributing to this growing discourse, we employ an identical framework with the objective of comparing the experiences of male victims to those of their female counterparts. As before, our analysis focuses on individuals reporting IPV for the first time, utilizing later-treated victims as the control group. The outcome variables are defined consistently with the specifications outlined in earlier sections. To the best of our knowledge, this study represents the first empirical investigation into the economic and social costs of IPV among men.

We present outcomes in Figure A.2 and Table B.4. In Panel A, we find similar patterns of divorce decisions where male once-reporters would also tolerate violence at the onset and make major divorce decisions only after reporting. Panel B demonstrates labor market performance; both male and female victims experience

negative consequences after the onset of violence. However, male once-reporters face challenges in returning to the labor market, unlike their female counterparts. In Panel C, we find statistically indistinguishable patterns in fertility rates between male and female victims.[19] Finally, we examine the frequency of depression outpatient visits in Panel D. Similar patterns are again documented where the visits surge in the reporting year and deteriorate afterward, albeit with a smaller magnitude. Per Table 1, this might not be surprising after all as the usage of depression clinics is smaller in the first place.

Subsequently, we analyze the patterns by stratifying the sample based on reporting age, with the corresponding results presented in Online Appendix Figure C.3 through Figure C.6. Overall, our findings suggest that younger male once-reporters experience less pronounced effects of IPV on both employment rates and fertility decisions, aligning with similar patterns observed among female once-reporters. However, due to limitations in sample size, we refrain from drawing definitive conclusions or conducting further disaggregation by educational attainment.

In summary, considering the similarities among fertility and divorce decisions when compared to females, it's not difficult to envision that IPV can result in equally, if not more, severe consequences for men. Moreover, given the societal pressures men often encounter in seeking help, it underscores the need for tailored support. For example, if males are not re-entering the labor market due to social stigma and discrimination, it would be beneficial to develop job training programs that are customized for specific roles. Likewise, the lower utilization of medical resources observed may also be attributed to similar reasons, warranting special assistance to be provided to reluctant male victims.

6.3 Heterogeneity in Reporting Behavior

To this point, our analysis has not explicitly focused on reporting behavior, incorporating factors related to it solely as covariates in estimating the effects of violence and reporting. For example, as demonstrated in Section 7.1, our primary

[19] The fertility definition for male victims is the same as for female victims. The outcome variable, *Fertility Rate*, is defined as 1 if a male who reported violence appears as a father in our data. Such relationships are identified through the National Health Insurance (NHI) data, which records the parental ID for all newborns.

results remain largely unchanged when covariates such as the reporting channel or the duration of IPV are included. Nevertheless, these aspects represent important areas of inquiry in their own right. While a detailed investigation into the determinants of reporting behavior lies beyond the scope of this paper, we offer preliminary analyses of reporting sources and timing, reserving a more rigorous causal exploration for future research.

6.3.1 Who Report Through What Channel?

To answer the question, we first present descriptive statistics in Table B.5, which reveal that, compared to women reporting a single incident through hospitals or helplines, those reporting through police channels tend to be older, have lower socioeconomic status, and possess lower levels of education. Additionally, they have fewer children under the age of 12 on average. Similarly, the perpetrators reported through police channels are observed to be older, less affluent, and less educated. To further investigate these patterns, we conduct a multinomial logit regression analysis, with results displayed in Table B.6. The regression analysis largely confirms the descriptive findings. For example, the probability of reporting through the police increases by 0.2% for each additional year of age of the reporting individual.

In the subsequent analysis, we estimate both violence and report effects across various reporting channels. Both Figure A.3 and Table B.7 provide insights into these relationships. Panel A consistently demonstrates that violence adversely impacts employment rates across all reporting channels. However, only individuals reporting through hospitals or helplines exhibit a capacity to re-enter the labor market. Similarly, Panel C reveals analogous patterns concerning fertility choices. Victims who report through hospitals or helplines are more likely to reduce their fertility rates compared to those who report through the police. This disparity is likely due to differences in demographic characteristics, as victims reporting to the police tend to be older. As elaborated in Sections 5.1 and 5.3, younger women who report violence are more likely to adjust their fertility decisions and are better positioned to re-enter the labor force.

Synthesizing the findings from the analyses above, it is crucial to recognize that, within the context of Taiwan, relying exclusively on data from a single re-

port channel may introduce selection bias. While this might explain some of the differences between Taiwan and countries relying on police data, we also acknowledge that reporting behavior can vary between countries due to cultural or logistical factors, which necessitates caution when extending these results to other contexts.

6.3.2 Who Delayed Reporting?

We address this issue by estimating an OLS regression model. The analysis incorporates victim characteristics, including reporting age, years of education, employment status, and income, alongside perpetrator characteristics, such as years of education and income. Additionally, the reporting channel utilized is considered. We run the following regression:

$$\text{Duration}_i = \alpha_0 + \alpha_1 \text{ReportAge}_i + \alpha_2 \text{Edu}_i + \alpha_3 \text{ChildUnder12}_i + \Gamma X_i + \epsilon_i, \quad (2)$$

where Duration_i is the year between IPV and reporting; ReportAge_i is the age when the report happened; Edu_i is individual education year; ChildUnder12_i is the number children under age 12. X_i includes three kinds of covariates. First, we consider employment-related variables such as a work dummy or the income level quartile. Second, we control for report channel. Third, we incorporate perpetrator characteristics such as education year or income level. All labor market outcomes are measured one year before reporting.

The results, presented in Table B.8, indicate that younger individuals and those without children experience shorter durations of IPV before reporting. Higher levels of education and income among victims are also associated with a reduced reporting duration. In contrast, perpetrator characteristics show no significant relationship with the duration. Notably, the reporting channel is significantly associated with the time to reporting, with individuals who report incidents through the police exhibiting shorter durations between experiencing violence and seeking intervention. It is worth noting that this framework does not adequately address omitted variable bias, and we do not intend to interpret these results causally. Nonetheless, this descriptive analysis contributes to a deeper understanding of victims' reporting behavior.

In addition to examining the duration, we focus on individuals who report IPV two years afterward and compare violence and report effects to our primary results. Although statistical power for this subgroup is limited, the observed patterns are consistent with our main labor, divorce, and fertility decisions. As shown in A.4, labor market outcomes initially deteriorate following IPV but demonstrate signs of recovery after reporting. Divorce filings increase approximately one year after IPV, with a pronounced surge at the time of reporting. Fertility decisions appear to change approximately one year before reporting, with a significant decline in fertility rates observed at the time of reporting. Regarding patterns of depression-related visits, we refrain from drawing definitive conclusions due to the limitations of the event study framework for analyzing this outcome, as highlighted in Panel D of Figure A.4. Finally, a comparison of the red and blue lines across Panels A to C reveals relatively consistent patterns, indicating that heterogeneity in IPV duration is unlikely to undermine the robustness of our primary specification.

An intriguing subgroup in this study consists of individuals who report IPV immediately after its occurrence. These "instant reporters" are excluded from the primary analysis due to the difficulty of disentangling violence and report effects. To further investigate, we analyze the four main outcomes for the 11.6% of female once-reporters who report IPV immediately, as illustrated in Figure A.5. Despite the limited sample size, the findings reveal broadly consistent patterns with the estimated report effects observed in the main analysis. Notably, focusing on employment rates, we observe that the magnitude of the positive report effect and the likelihood of re-entering the labor market are nearly double those in the primary results. This pattern plausibly suggests that individuals who report IPV immediately may possess greater determination or capability compared to other groups.

Combining the above analyses, we conclude that, while there are observable differences between individuals who report IPV sooner versus later, there are no significant differences in the observed outcomes attributable to the timing of reporting—whether it occurs immediately or up to two years later.

7 Robustness Check

To bolster the robustness of our analysis and the interpretation of treatment effects, we conduct several complementary checks. First, we incorporate different covariates to assess whether additional variables alter our findings. Second, we employ an alternative event study specification to ensure the appropriateness of our chosen framework and explore potential heterogeneity across cohorts. Third, we address the challenge of correlated events by applying two distinct strategies to isolate the treatment effects. Fourth, we explore different levels of data aggregation (monthly and quarterly) to examine the consistency of our results. Fifth, we investigate the impact of clustering standard errors at different levels, considering potential correlations within reporting channels in the same county. Lastly, we examine once-reporters who are non-surveyed and alternatively defined.

7.1 Alternative Covariates

We analyze the impact of incorporating alternative covariates, in addition to education level and year of birth, under our primary IPW DiD estimators. The first set of covariates encompasses characteristics and outcomes related to perpetrators, including age, educational attainment, income level, and employment status. The second set focuses on measures of IPV severity and the type of violence (physical or not). As illustrated in Online Figure C.7, the inclusion of these covariates does not substantively alter the main findings.

A particularly challenging category of covariates pertains to reporting behavior, as it may involve issues of selection bias and unobserved characteristics. For example, individuals who report incidents earlier may differ systematically from those who report later, and those who report to the police may differ from individuals who report elsewhere. To address this potential concern, we employ two strategies. First, we undertake a comprehensive analysis of reporting behavior, as discussed in greater detail in Section 6.3. In particular, we limit the sample to individuals who report incidents two years after experiencing IPV, thereby removing any variation associated with reporting timing. Additionally, we decompose the main sample by report channel and compare each estimate.

Second, we simply incorporate these variables as covariates in the analysis,

treating them in the same manner as standard covariates, and present the results in Online Appendix Figure C.8. Notably, the findings closely align with our baseline results, which include only the two core covariates, providing reassurance regarding the robustness of our approach.

7.2 Alternative Event Study Specification

As reviewed by Roth et al. (2023), there are numerous methods for estimating dynamic average treatment effects. We use the approach proposed by Callaway and Sant'Anna (2021), employing later-treated groups as controls in our main specification. This choice is motivated by our belief that individuals treated later are comparable to the treatment group and that heterogeneous treatment effects may exist across cohorts due to evolving factors such as public awareness, policy environments, and changes in labor and marriage markets over time.

To evaluate the robustness of our findings, we estimate the effects using the canonical two-way fixed effects model. Leveraging the decomposition method proposed by Goodman-Bacon (2021), we observe that OLS assigns 28.5% of the weights as negative. This result highlights the critical importance of employing the Callaway and Sant'Anna (2021) approach. To address the issue of negative weights, we further adopt the methodology and provide a detailed comparison in Online Online Appendix Figure C.9. We find that violence effects are fairly alike while reporting effects are qualitatively similar.[20]

7.3 Correlated Events

Our two events—violence and reporting—are highly correlated. This creates the econometric challenge in which outcomes for individuals in the control group who have not yet reported IPV may still be influenced by ongoing exposure to IPV.

To address this issue, we conduct two additional analyses: (1) using the never-treated group as the control group, and (2) employing a multiple-event study technique.

[20] The differences in reporting effect may arise from the different comparison groups: Sun and Abraham (2021) relies exclusively on the last-treated cohort as the comparison group, whereas Callaway and Sant'Anna (2021) incorporates all later-treated cohorts as the control group.

Never-Treated as Comparison. While the never-treated group is less similar to the treatment group as they never experience the event, these individuals are not influenced by ongoing exposure to IPV. To examine the results using the never-treated group as controls, we draw 3 never-treated individuals for each treated individual, matched on birth year and level of education.

We replicate the estimation outlined in Equation 1, modifying only the composition of the control group. Specifically, for each cohort and event time, the control group comprises matched never-treated individuals, with the same IPW procedure applied. This alternative specification is referred to as "Matched DiD," reflecting the use of matched control groups, whereas our primary specification is labeled "Staggered DiD," highlighting the staggered nature of treatment timing. The results, displayed in Figure A.6 and Table B.1, indicate that employing never-treated individuals as controls yields findings that are largely consistent with those from our main specification. Note that the interpretation of estimates using the never-treated should differ from that using the later-treated, as the comparison groups differ.

Double DiD. To address the correlation between sequential events in event study analyses, we adopt the methodology proposed by Tsai (2024). This approach explicitly accounts for the sequential nature of events and identifies treatment effects by employing units not yet exposed to either event as controls. The estimation results, displayed in Online Appendix Figure C.10, reveal that the findings from this method are qualitatively consistent with those of our primary specification.[21]

7.4 Alternative Observation Aggregation

For labor market and health outcomes, it is possible to observe monthly data. In Online Appendix Figure C.11, we provide a more granular analysis by examining outcomes at both the monthly and quarterly levels.

First, we analyze the effect of reporting on the employment rate at the monthly

[21] This method is referred to as "double DiD" because it simultaneously incorporates both events in the estimation process, rather than treating the other event as fixed, as in our main specification.

and quarterly levels. Overall, we observe similar patterns, where female once-reporters tend to return to the labor market shortly after reporting, with the effect being both immediate and persistent. The monthly-level analysis shows that this effect begins almost immediately, starting in the first month after reporting.

Similarly, we expand our analysis of the depression visit indicator to both the monthly and quarterly levels. The results reveal a consistent pattern: female once-reporters experience a significant increase in depression visits around the time of reporting, followed by a sharp decline. Interestingly, the monthly-level analysis indicates a short window of approximately two months during which a positive treatment effect is observed.

7.5 Alternative Level of Clustering

In our main specification, the standard errors are clustered at the individual level. One may think that there could be some correlation within the same reporting channel in the same county. We further perform a robustness check by clustering on counties by channel and show the results in Online Appendix Figure C.12; reassuringly, the results remain similar.

7.6 Missing Value on Violence Onset

In Section 3.2.1, we note that only 63% of female once-reporters responded to the survey question, "How long has the violence lasted?" Consequently, the estimation of the effect of violence is restricted to this subset of respondents. Table B.5 reveals that the response rate is highest at 74.9% among individuals who reported incidents through the police and substantially lower for those reporting via hospitals or helplines. We posit that, although officials are mandated to administer these surveys, challenges such as staffing shortages (e.g., insufficient social workers in hospitals) and difficulties in locating victims (e.g., by helpline staff) may impede survey completion.

To mitigate the issue, we undertake two approaches. First, as illustrated in Table B.9, Panels A through E, respondents and non-respondents exhibit broadly similar characteristics, which enhances confidence in the representativeness of our violence effect estimates. Second, we evaluate whether the surveyed sample ade-

quately represents the broader population. Specifically, we estimate the reporting effect within the surveyed group and present the results in Online Appendix Figure C.13. Consistent patterns are shown across all four outcomes, thereby reinforcing the validity of our causal estimates. Collectively, these findings alleviate concerns regarding representativeness and the reliability of the violence effect estimation.

7.7 Alternative Definition of Once-Reporter

Since we do not have data prior to 2012, we can only define "once-reporter" as reporters who appeared only once from 2012 to 2018. However, our definition is somewhat limited as we cannot make sure they didn't report prior to 2012; as a result, we define female once-reporters alternatively as women who reported only once in 2014-2018 and zero time in 2012-2013 to further secure our definition. Reassuringly, we find similar patterns between our main results and the alternative definition across the four outcomes in Online Appendix Figure C.14. This consistency lends further confidence to the robustness of our definition of once-reporters in the primary analysis.

8 Conclusion

This paper causally estimates the costs associated with IPV, leveraging a merged dataset that encompasses all reported cases of IPV in Taiwan from 2012 to 2018 along with various administrative records. Using an event-study design, we investigate both the violence effect and report effect across 4 sets of outcome variables: labor market performance, divorce decisions, fertility decisions, and mental health. For female once-reporters, we find evidence of a negative impact on employment rates when IPV initially occurs; however, as these victims reach out through official channels, there is a gradual return to the labor market. This effect is predominantly driven by young and educated individuals. Additionally, we demonstrate that labor market patterns align plausibly with divorce and fertility decisions, suggesting a potential interplay among these choices. Furthermore, we document the IPV costs among men, which exhibit similar patterns to those observed in women despite less pronounced patterns; this set of evidence contributes to the literature as there is limited empirical evidence on men so far. It

is important to note that the estimates in this study, with later-treated groups serving as control groups, pertain to an interpretation of ATT and may not directly apply to IPV victims who do not report. We emphasize the need for future research to explore the comprehensive costs of IPV, particularly in undocumented cases, where welfare policies and assistance programs can effectively address and eradicate violence between intimate partners.

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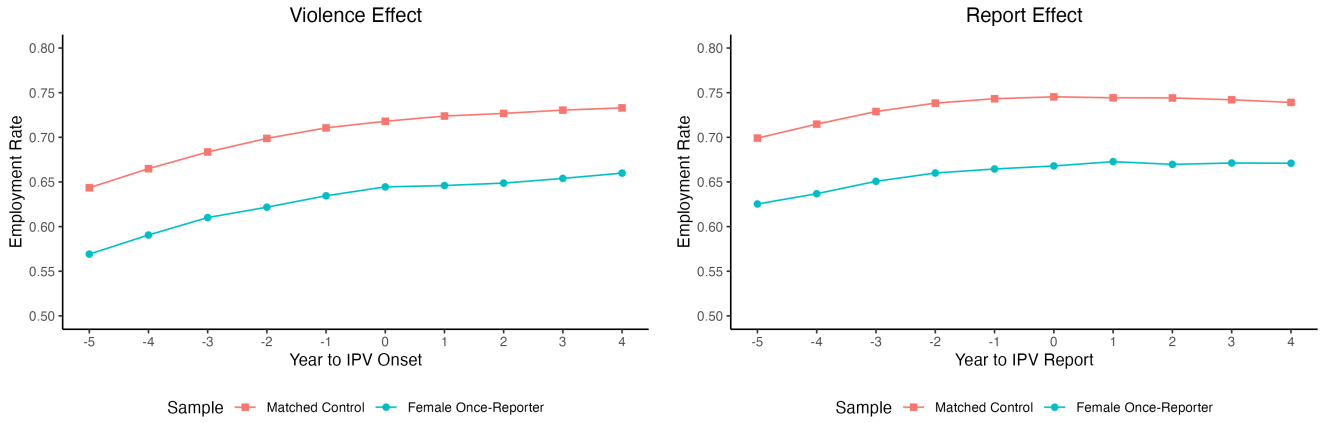
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A Appendix Figures

Panel A: Employment Rate



Panel B: Divorce Decision

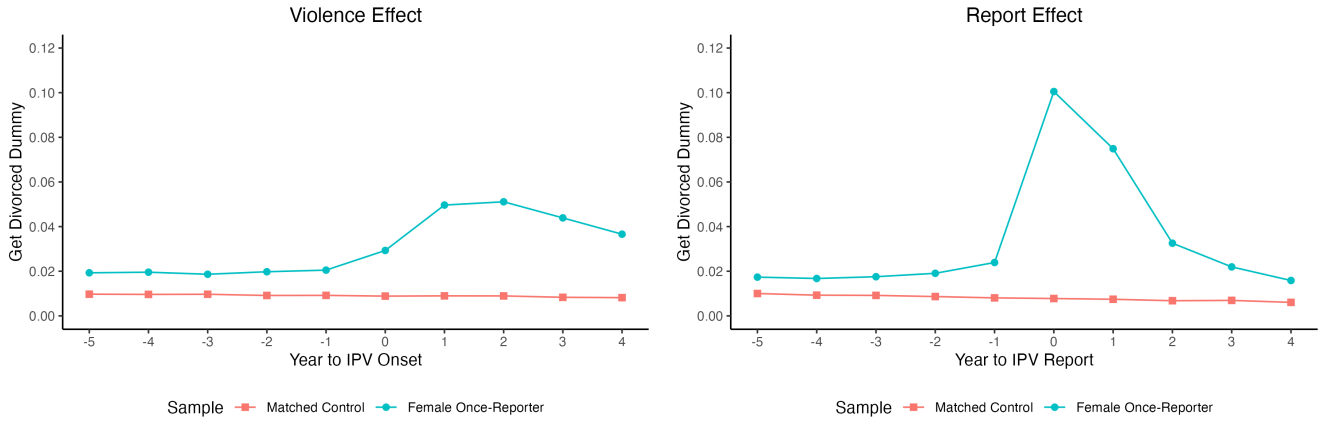
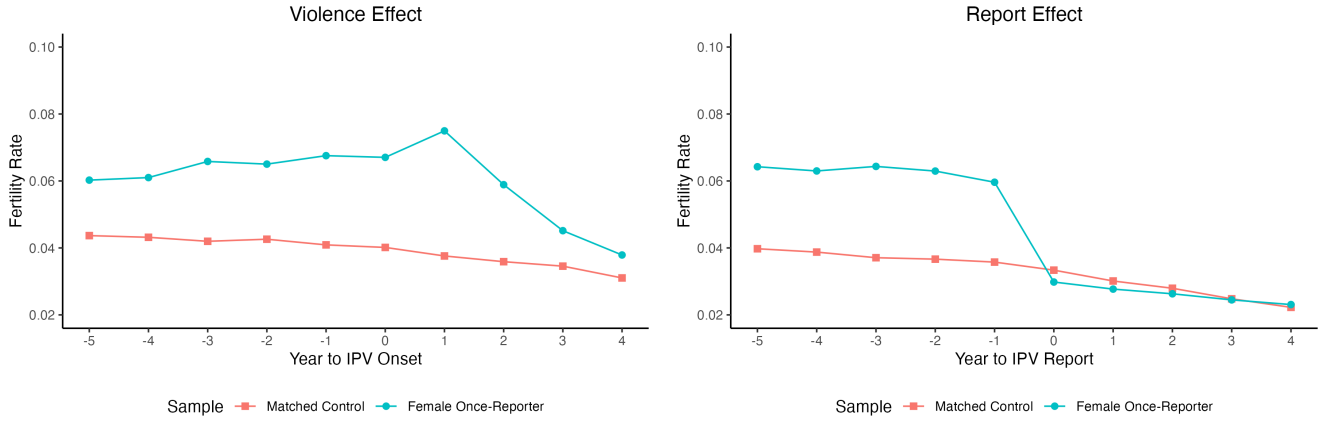
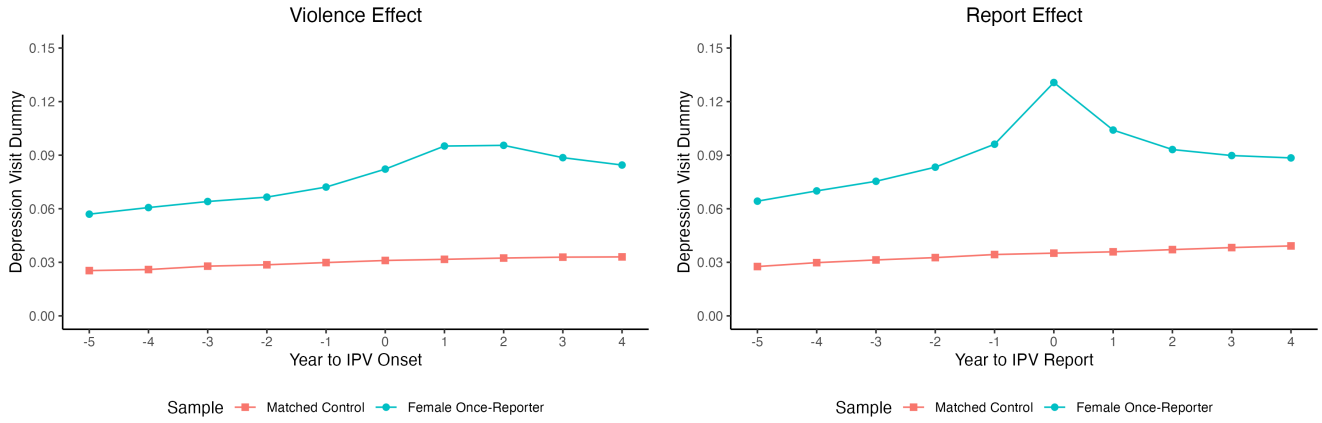


Figure A.1: Raw Data Among Female Once-Reporters Across Outcomes

Panel C: Fertility Decision



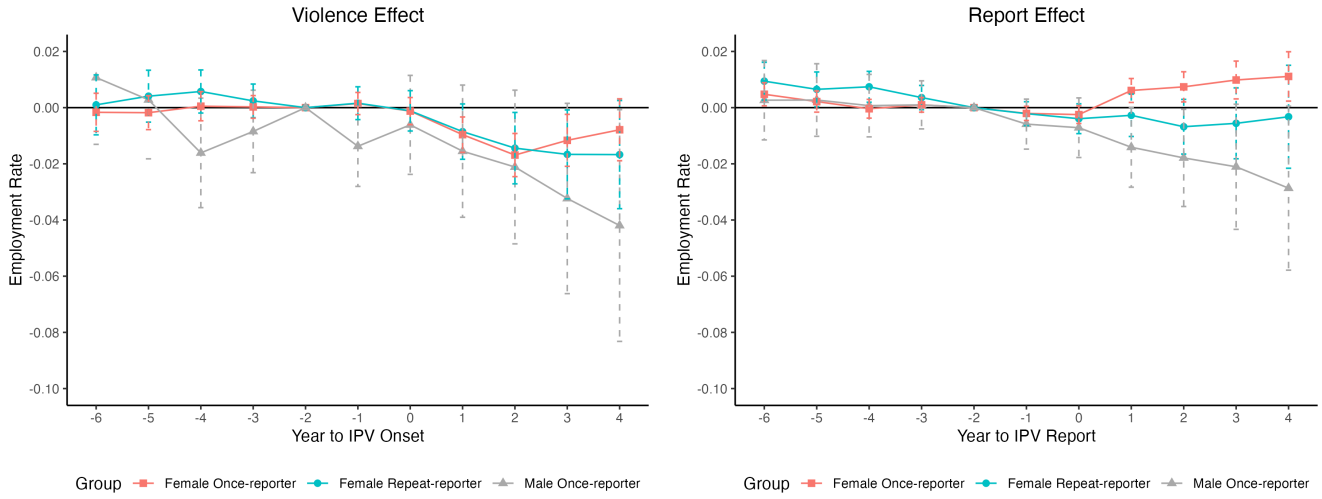
Panel D: Depression Outpatient Visit



Notes: This figure depicts the raw data for our four main outcomes. The blue line consists of 90,604 people female once-reporters; the red line includes another 271,812 matched non-victims/non-reporters based on two criteria - education year and year of birth.

Figure A.1: Raw Data Among Female Once-Reporters Across Outcomes (Continued)

Panel A: Employment Rate



Panel B: Divorce Decision

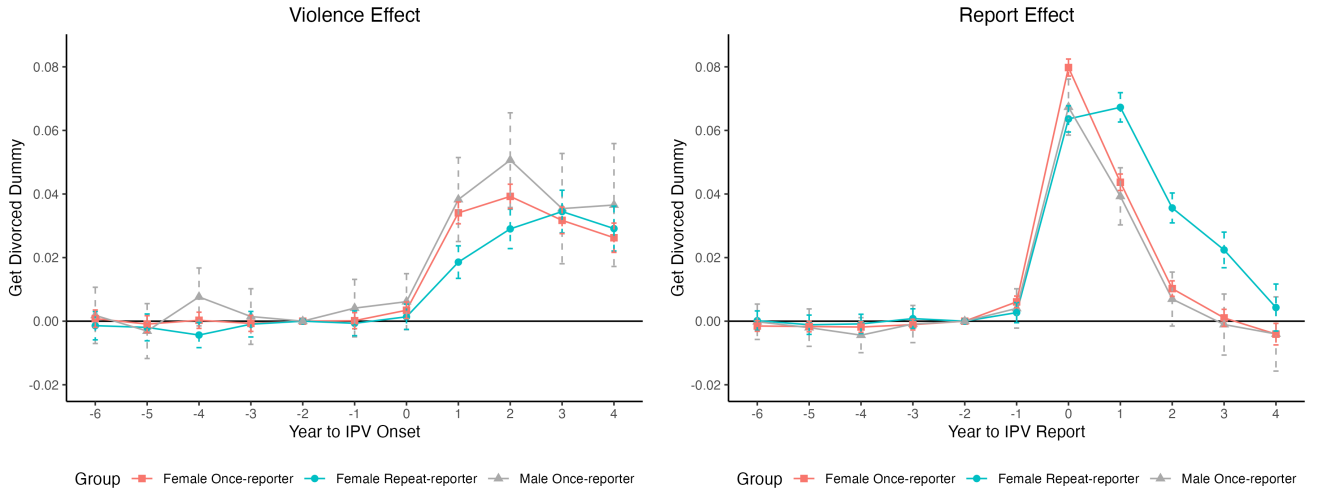
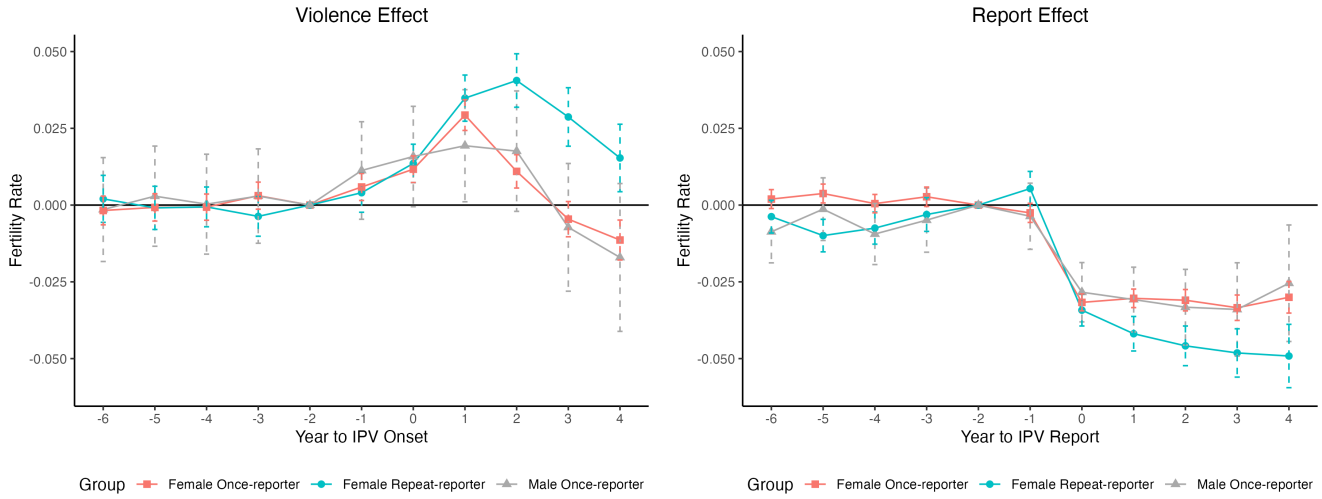
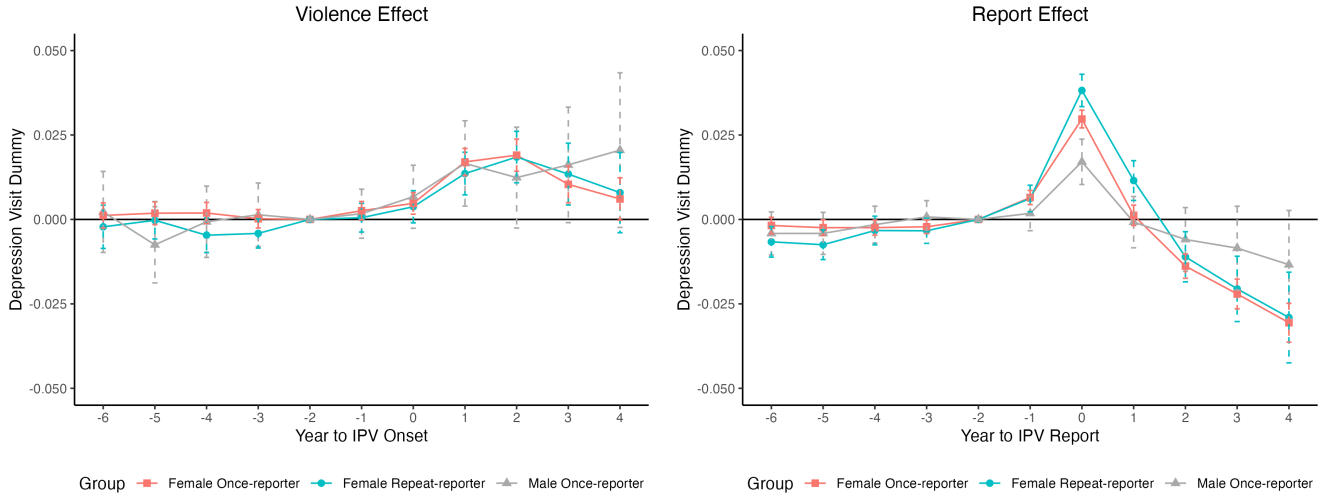


Figure A.2: Comparison Among Victim Groups

Panel C: Fertility Decision



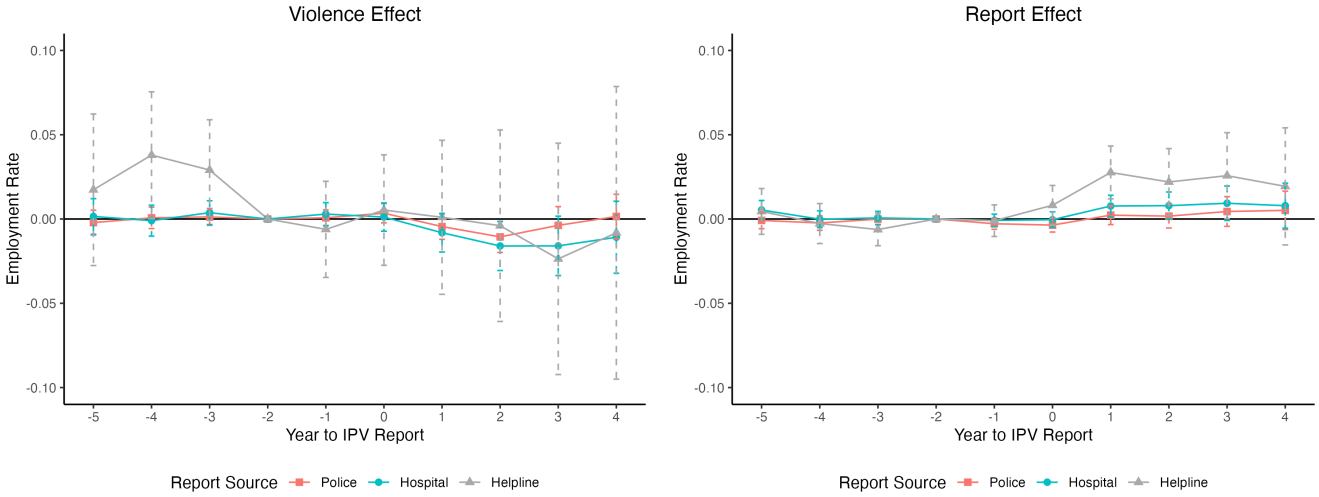
Panel D: Depression Outpatient Visit



Notes: The figure compares four main outcomes across female once-reporters, female repeat-reporters, and male once-reporters. Dash lines are 95% confidence interval of the estimates.

Figure A.2: Comparison Between Victim Groups (Continued)

Panel A: Employment Rate



Panel B: Divorce Decision

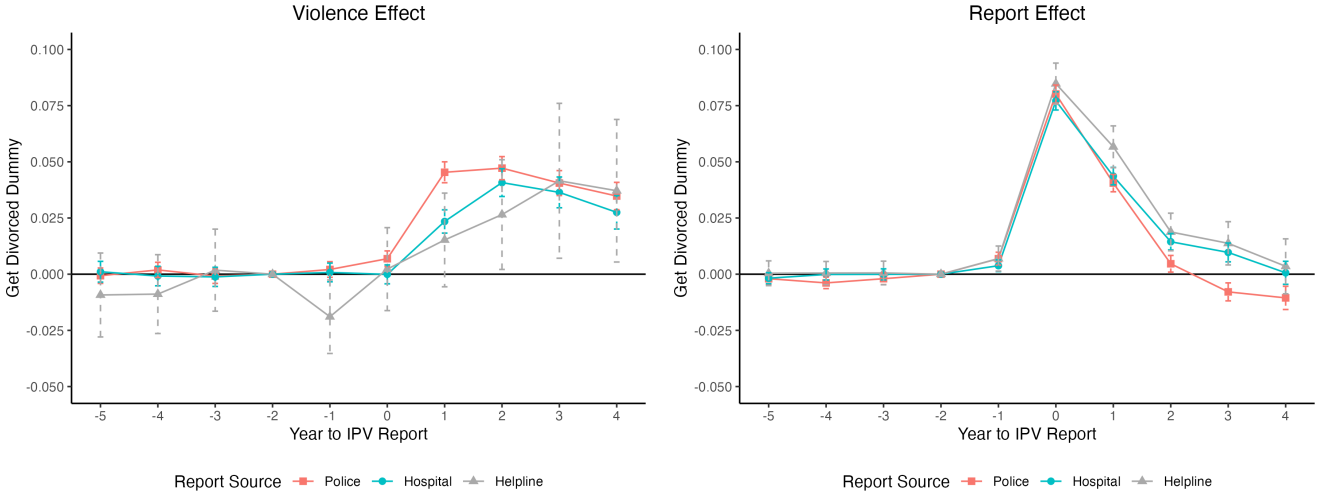
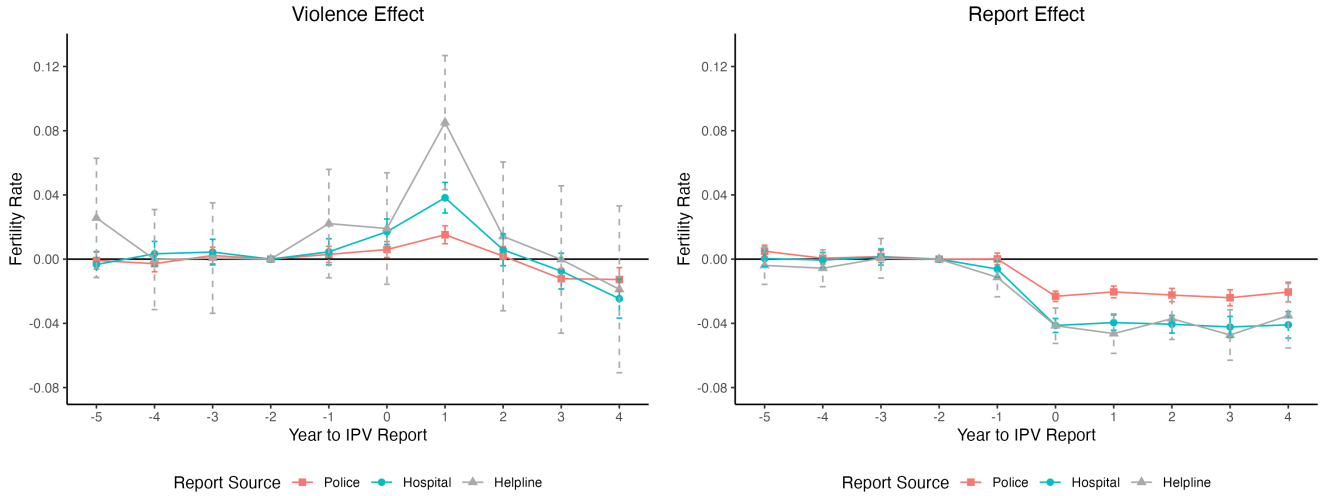
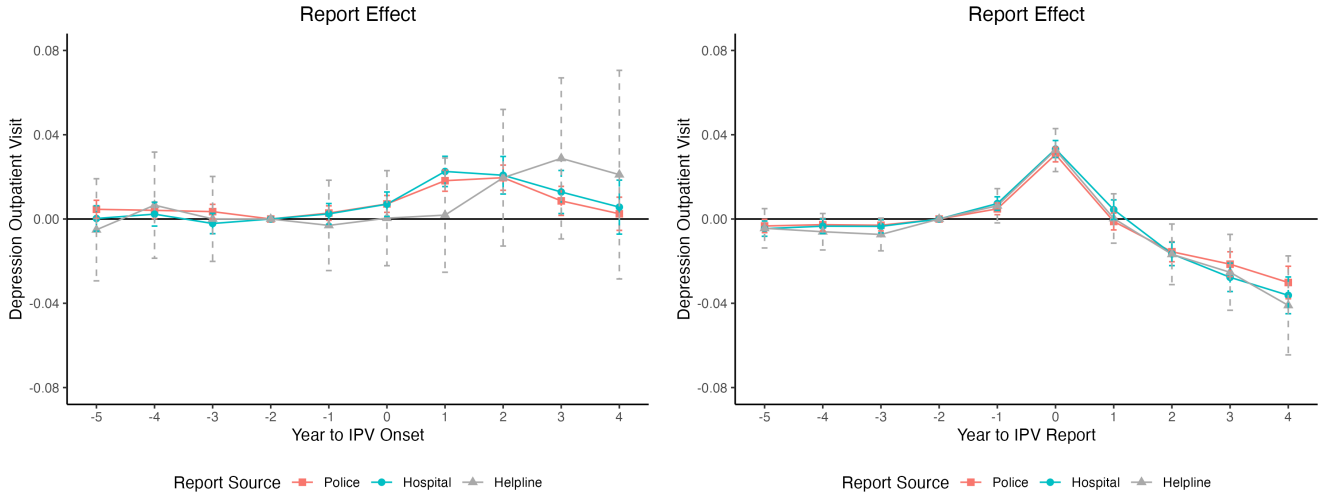


Figure A.3: Heterogeneity in Report Channel

Panel C: Fertility Decision



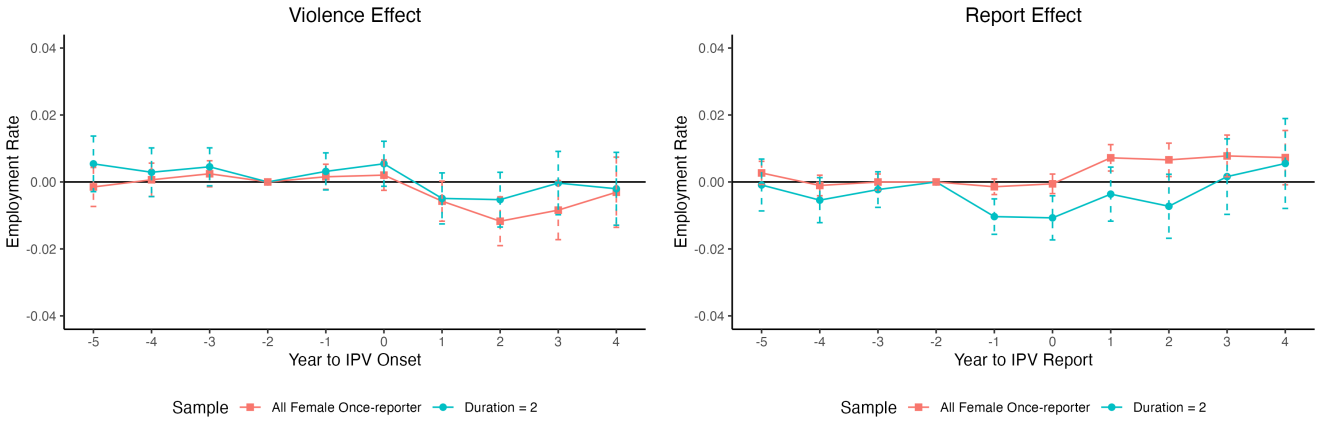
Panel D: Depression Outpatient Visit



Notes: The figure compares four main outcomes in three main report channels. Dash lines are 95% confidence interval of the estimates.

Figure A.3: Heterogeneity in Report Channel (Continued)

Panel A: Employment Rate



Panel B: Divorce Decision

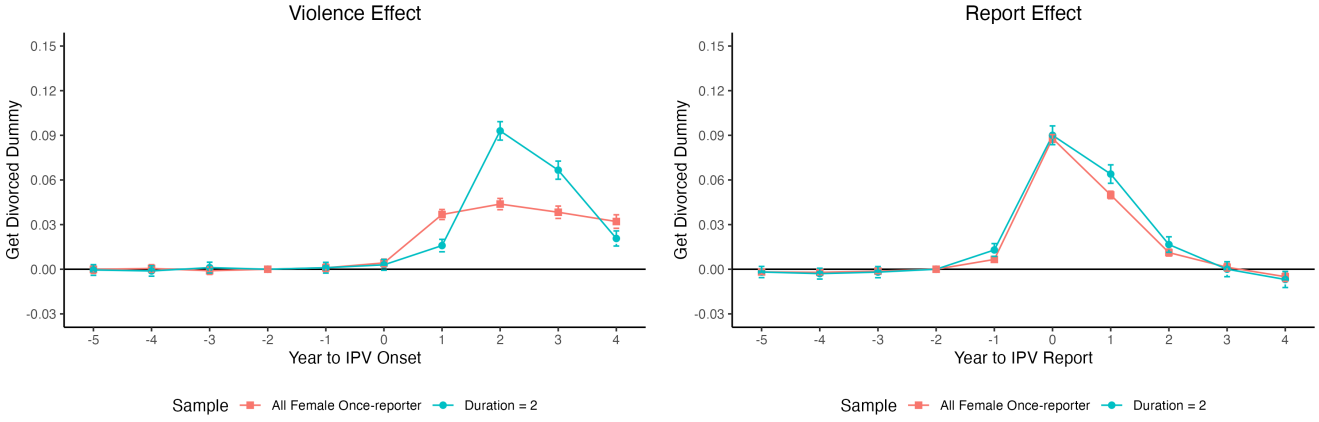
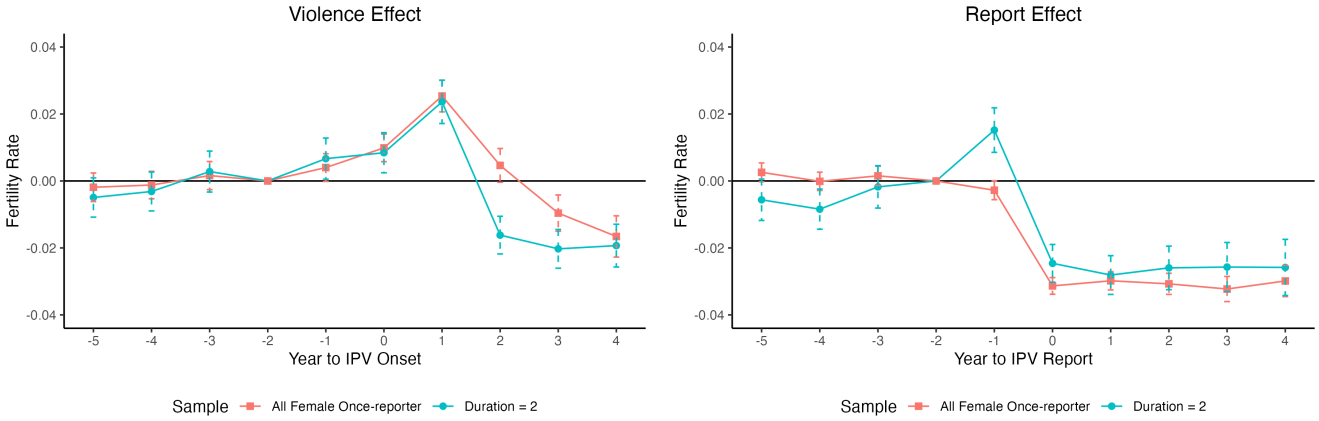
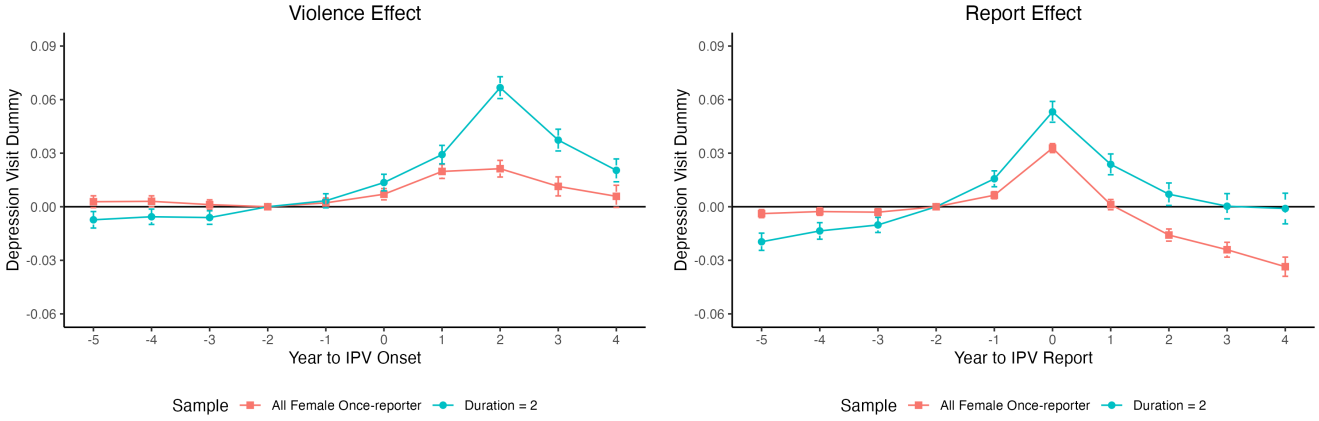


Figure A.4: Comparison Between All Female Once-reporters and Duration = 2

Panel C: Fertility Decision

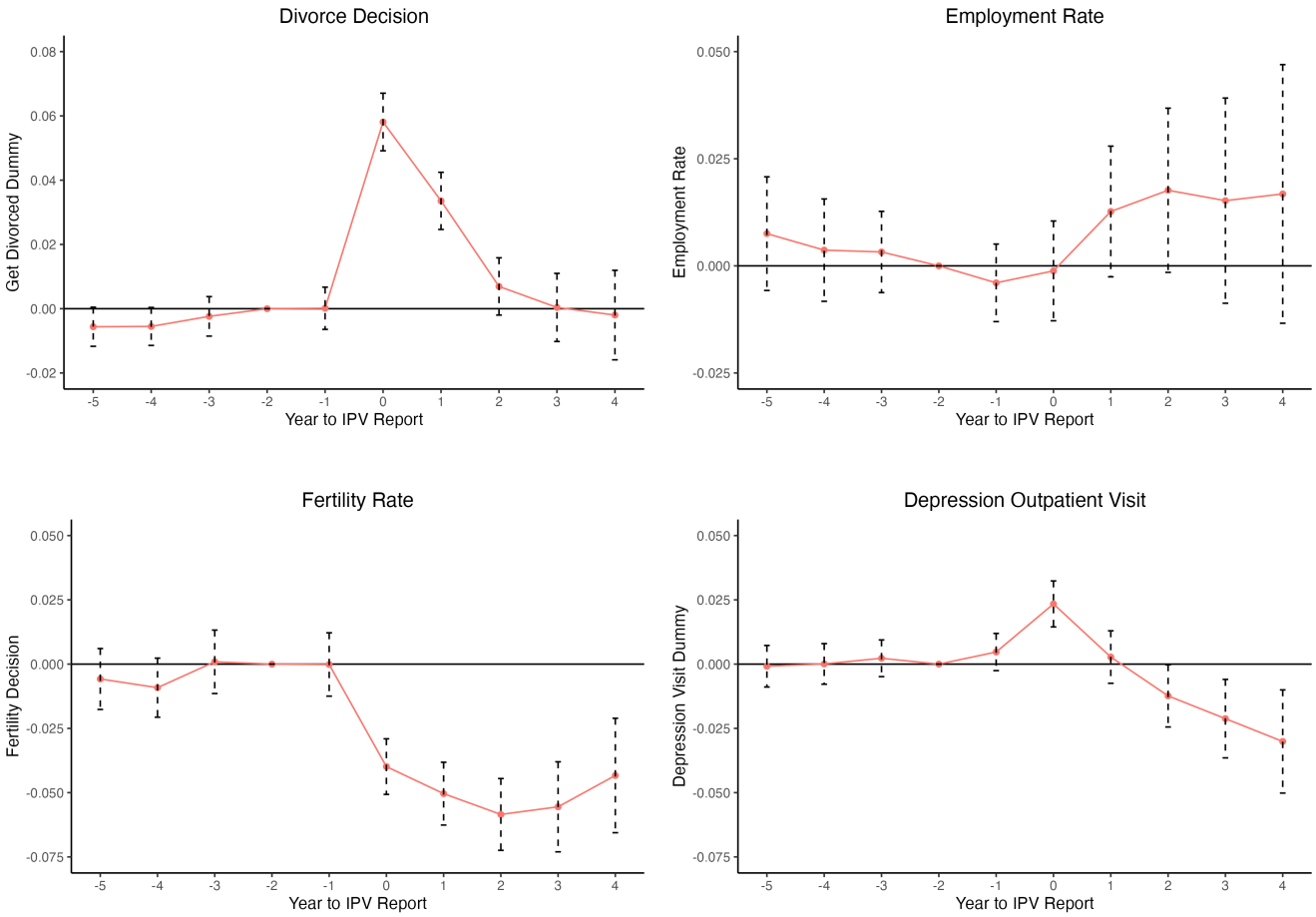


Panel D: Depression Outpatient Visit



Notes: Under staggered DiD, this figure compares four main outcomes between all female once-reporters and those who report two years after IPV happened. Dash lines are 95% confidence interval of the estimates.

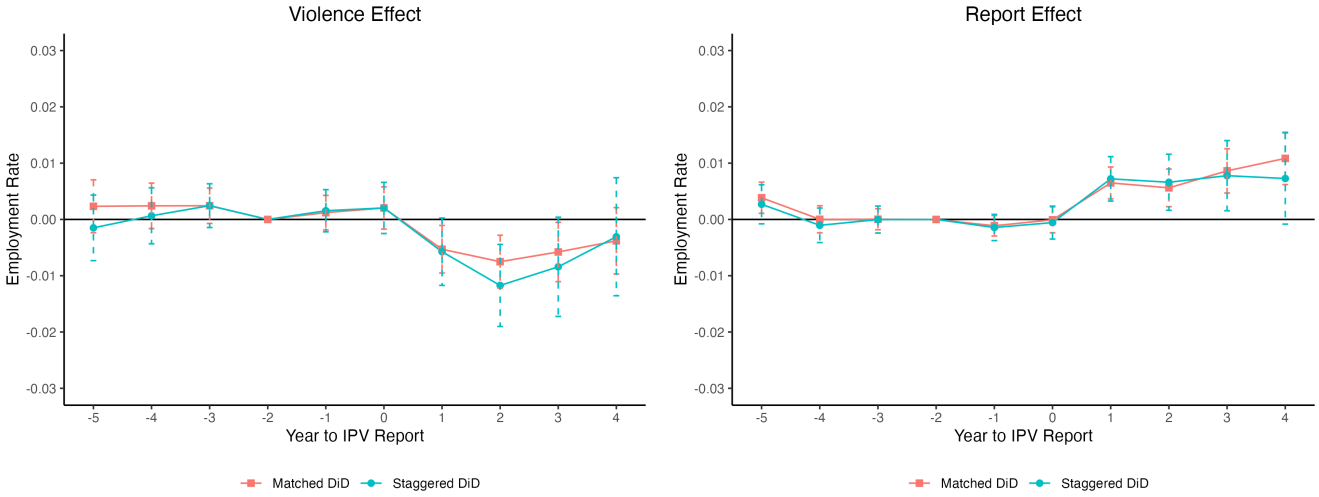
Figure A.4: Comparison Between All Female Once-reporters and Duration = 2 (Continued)



Notes: The figure presents four main outcomes among people who reported IPV immediately after it happened. Dash lines are 95% confidence interval of the estimates.

Figure A.5: Report Effect Among Instant Reporters

Panel A: Employment Rate



Panel B: Divorce Decision

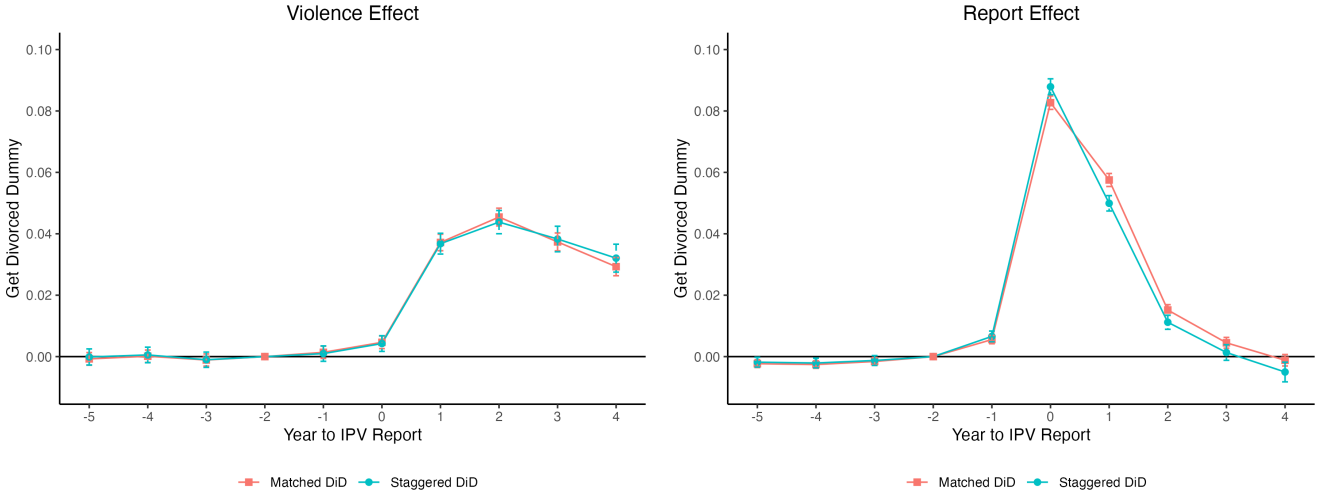
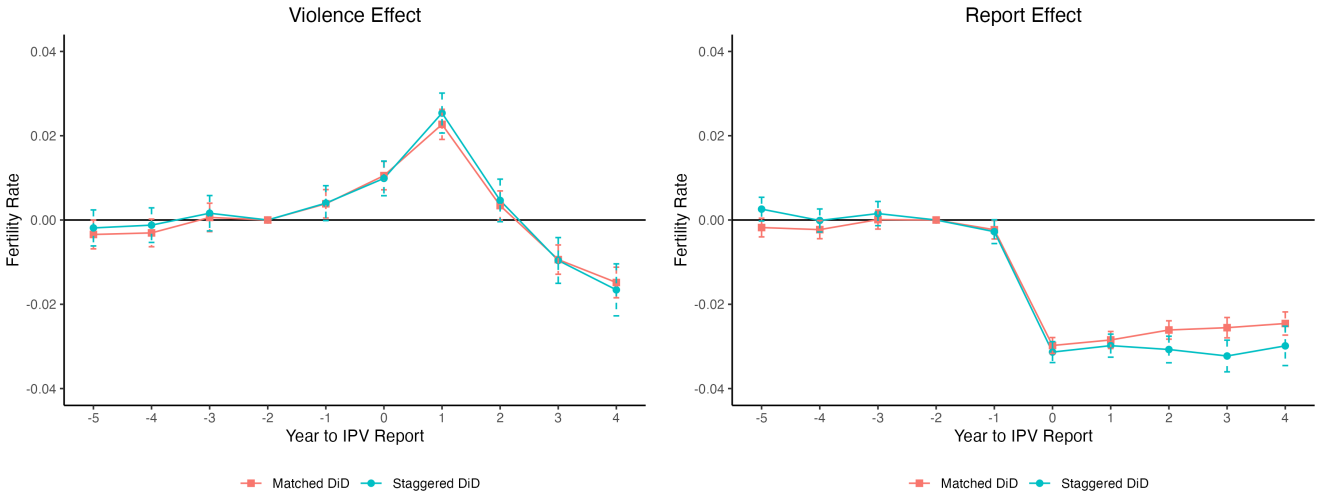
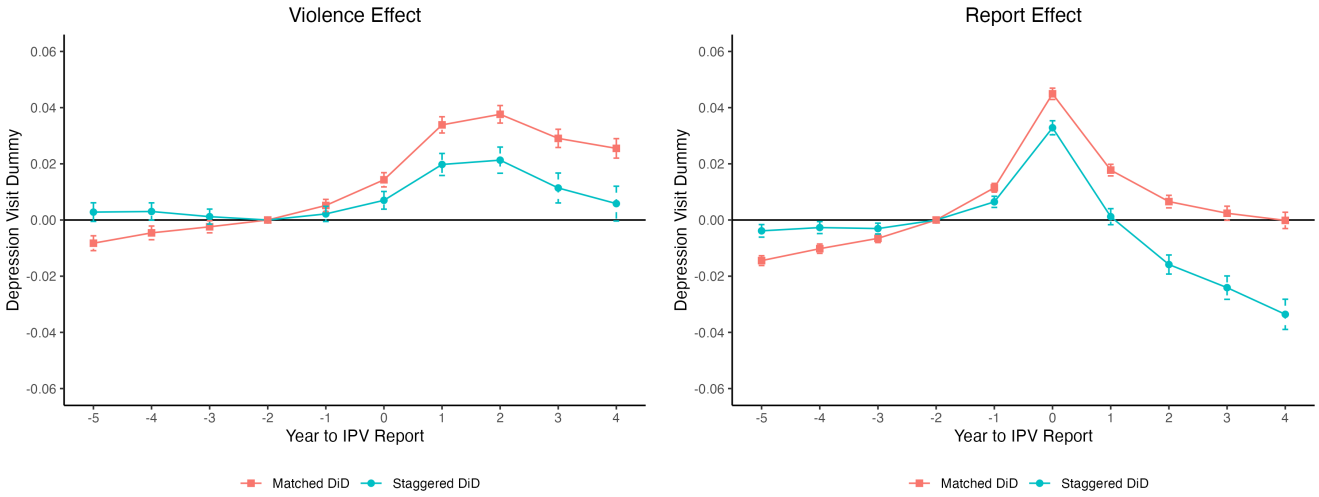


Figure A.6: Comparison Between Staggered and Matched DiD

Panel C: Fertility Decision



Panel D: Depression Outpatient Visit



Notes: This figure compares staggered DiD and matched DiD across four main outcomes among all female once-reporters. Staggered DiD uses the later-treated as control group while matched DiD uses the never-treated as control group. Dash lines are 95% confidence interval of the estimates.

Figure A.6: Comparison Between Staggered and Matched DiD (Continued)

B Appendix Tables

Table B.1: Main Result

		Dynamic Violence Effect					Static		Dynamic Report Effect					Static
Relative Time To Event	*Mean in -2*	*t = 0*	*t = 1*	*t = 2*	*t = 3*	*t = 4*	*t ≥ -1*	*Mean in -2*	*t = 0*	*t = 1*	*t = 2*	*t = 3*	*t = 4*	*t ≥ -1*
A. Employment Rate														
Staggered DiD	0.614	0.002	-0.006*	-0.011**	-0.008*	-0.003	-0.003	0.656	0.000	0.007***	0.007***	0.008**	0.007	0.005**
		(0.002)	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)		(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.002)
Matched DiD	0.618	0.002	-0.005**	-0.008***	-0.006**	-0.004	-0.001	0.660	0.000	0.006***	0.006***	0.009***	0.011***	0.006***
		(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)		(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
B. Get Divorced Dummy														
Staggered DiD	0.020	0.004***	0.037***	0.044***	0.038***	0.032***	0.030***	0.019	0.088***	0.050***	0.011***	0.000	-0.005***	0.039***
		(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Matched DiD	0.020	0.005***	0.037***	0.045***	0.037***	0.029***	0.027***	0.019	0.083***	0.058***	0.015***	0.004***	-0.001	0.035***
		(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
C. Fertility Rate														
Staggered DiD	0.063	0.010***	0.025***	0.005*	-0.010***	-0.017***	-0.001	0.064	-0.031***	-0.030***	-0.031***	-0.032***	-0.030***	-0.031***
		(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)		(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
Matched DiD	0.062	0.011***	0.023***	0.003*	-0.010***	-0.015***	-0.005***	0.063	-0.030***	-0.028***	-0.026***	-0.026***	-0.025***	-0.027***
		(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
D. Depression Visit Dummy														
Staggered DiD	0.064	0.007***	0.020***	0.020***	0.011***	0.006*	0.010***	0.082	0.033***	0.001	-0.016***	-0.024***	-0.033***	-0.002
		(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)		(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)
Matched DiD	0.066	0.014***	0.034***	0.038***	0.029***	0.026*	0.027***	0.083	0.045***	0.018***	0.007***	0.002*	0.000	0.016***
		(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
E. Regression Information														
Staggered DiD Unique *ID NO.*	41,057							90,604						
Regression Observation	5,274,844							7,845,334						
Matched DiD Unique *ID NO.*	164,228							362,416						
Regression Observation	41,493,172							53,906,188						

Notes: This table summarizes all staggered DiD and matched DiD estimates across four main outcomes. Staggered DiD uses the later-treated as control group while Matched DiD uses the never-treated as control group. The mean in period -2 is also presented. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table B.2: Heterogeneity in Reporting Age

Sample	Average Violence Effect ($t \geq$ -1) *Staggered DiD Coef.*	*Mean in* $t = -2$	Average Report Effect ($t \geq$ -1) *Staggered DiD Coef.*	*Mean in* $t = -2$
Panel A: Employment Rate				
Young Victim (25-35)	-0.008	0.566	0.010**	0.628
	(0.006)		(0.004)	
Mid-aged Victim (36-45)	-0.006	0.633	0.002	0.659
	(0.006)		(0.003)	
Old Victim (46-55)	0.001	0.664	0.001	0.687
	(0.007)		(0.004)	
Panel B: Get Divorced Dummy				
Young Victim (25-35)	0.031***	0.021	0.060***	0.022
	(0.002)		(0.002)	
Mid-aged Victim (36-45)	0.030***	0.022	0.043***	0.021
	(0.002)		(0.002)	
Old Victim (46-55)	0.017***	0.015	0.026***	0.012
	(0.003)		(0.002)	
Panel C: Fertility Rate				
Young Victim (25-35)	0.007	0.099	-0.074***	0.130
	(0.004)		(0.003)	
Mid-aged Victim (36-45)	0.002	0.059	-0.017***	0.050
	(0.003)		(0.002)	
Old Victim (46-55)	0.000	0.005	0.000	0.001
	(0.001)		(0.000)	
Panel D: Depression Visit Dummy				
Young Victim (25-35)	0.006*	0.047	0.003	0.062
	(0.003)		(0.002)	
Mid-aged Victim (36-45)	0.009**	0.071	-0.002	0.087
	(0.004)		(0.002)	
Old Victim (46-55)	0.019***	0.083	0.003	0.102
	(0.005)		(0.003)	
Panel E: Regression Information				
Young Victim Unique *ID NO.*	16,099		31,384	
Regression Observation	1,953,530		2,397,714	
Mid-aged Victim Unique *ID NO.*	16,191		36,007	
Regression Observation	2,000,9422		2,376,280	
Old Victim Unique *ID NO.*	8,767		23,213	
Regression Observation	1,057232		1,518,858	

Notes: This table investigates heterogeneity in reporting age across four main outcomes. Instead of a dynamic framework, a simple static framework is presented here where only before-after estimates are shown. The mean in period -2 is also presented. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table B.3: Heterogeneity in Education Among the Young

Sample	Average Violence Effect ($t \geq$ -1) *Staggered DiD Coef.*	*Mean in* $t = -2$	Average Report Effect ($t \geq$ -1) *Staggered DiD Coef.*	*Mean in* $t = -2$
Panel A: Employment Rate				
Middle School Or Below	-0.016	0.360	-0.004	0.405
	(0.013)		(0.009)	
High School	-0.003	0.544	0.017**	0.612
	(0.010)		(0.007)	
At Least College	-0.009	0.710	0.012*	0.791
	(0.010)		(0.006)	
Panel B: Get Divorced Dummy				
Middle School Or Below	0.020***	0.038	0.054***	0.031
	(0.006)		(0.004)	
High School	0.033***	0.022	0.061***	0.026
	(0.004)		(0.003)	
At Least College	0.035***	0.009	0.063***	0.012
	(0.003)		(0.002)	
Panel C: Fertility Rate				
Middle School Or Below	-0.012	0.110	-0.030***	0.108
	(0.009)		(0.007)	
High School	-0.001	0.107	-0.067***	0.122
	(0.007)		(0.005)	
At Least College	-0.011	0.083	-0.114***	0.155
	(0.007)		(0.006)	
Panel D: Depression Visit Dummy				
Middle School Or Below	0.000	0.058	0.002	0.077
	(0.008)		(0.006)	
High School	0.008	0.049	0.006	0.062
	(0.005)		(0.004)	
At Least College	0.008*	0.039	0.000	0.051
	(0.004)		(0.004)	
Panel E: Regression Information				
Middle School Or Below Unique *ID NO.*	3,613		7,129	
Regression Observation	437,496		543,270	
High School Unique *ID NO.*	6,220		12,360	
Regression Observation	744,986		899,138	
At Least College Unique *ID NO.*	6,266		11,895	
Regression Observation	771,048		955,306	

Notes: This table investigates heterogeneity in education among the young (age 25-55). Instead of a dynamic framework, a simple static framework is presented here where only before-after estimates are shown. The mean in period -2 is also presented. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table B.4: Heterogeneity in Victim Group

Sample	Average Violence Effect ($t \geq$ -1) *Staggered DiD Coef.*	*Mean in* $t = -2$	Average Report Effect ($t \geq$ -1) *Staggered DiD Coef.*	*Mean in* $t = -2$
Panel A: Employment Rate				
Female Once-reporter	-0.003	0.614	0.005**	0.656
	(0.004)		(0.002)	
Female Repeat-reporter	-0.008	0.524	-0.003	0.567
	(0.008)		(0.005)	
Male Once-reporter	-0.010	0.734	-0.016**	0.752
	(0.011)		(0.006)	
Panel B: Get Divorced Dummy				
Female Once-reporter	0.030***	0.020	0.039***	0.019
	(0.001)		(0.001)	
Female Repeat-reporter	0.023***	0.026	0.039***	0.025
	(0.003)		(0.002)	
Male Once-reporter	0.034***	0.016	0.038***	0.021
	(0.004)		(0.003)	
Panel C: Fertility Rate				
Female Once-reporter	-0.001	0.063	-0.031***	0.064
	(0.002)		(0.001)	
Female Repeat-reporter	0.016***	0.073	-0.048***	0.079
	(0.005)		(0.003)	
Male Once-reporter	0.000	0.056	-0.037***	0.067
	(0.007)		(0.004)	
Panel D: Depression Visit Dummy				
Female Once-reporter	0.010***	0.064	-0.002	0.082
	(0.002)		(0.001)	
Female Repeat-reporter	0.007	0.084	0.009**	0.115
	(0.006)		(0.004)	
Male Once-reporter	0.012**	0.034	0.001	0.042
	(0.005)		(0.003)	
Panel E: Regression Information				
Female Once-reporter Unique *ID NO.*	41,057		90,604	
Regression Observation	5,274,844		7,845,334	
Female Repeat-reporter Unique *ID NO.*	11,519		25,753	
Regression Observation	1,002,972		1,488,086	
Male Once-reporter Unique *ID NO.*	3,579		9,150	
Regression Observation	366,326		82,9074	

Notes: This table investigates heterogeneity across victim groups. Instead of a dynamic framework, a simple static framework is presented here where only before-after estimates are shown. The mean in period -2 is also presented. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table B.5: Summary Statistics by Report Channel Among Female Once-reporters

		Report Channel			
Sample: All Female Once-reporters	All	Police	Hospital	Helpline	Other
Panel A: Victim Demographics					
Mean Age	39.61	40.18	38.79	40.02	39.73
Mean Education Years	12.27	12.07	12.48	12.59	12.06
Depression Outpatient-visit	9.6%	9.3%	9.5%	10.8%	12.0%
Panel B: Social Worker Evaluation					
Labeled "Severe" & Provided Assistance	33.1%	34.2%	29.9%	36.0%	44.6%
IPV Severity Index (Out of 13)	3.66	3.64	3.80	2.81	3.71
Panel C: Perpetrator Statistics					
Mean Age	42.84	43.19	41.66	43.47	43.16
Mean Education Years	11.72	11.53	12.21	12.04	11.50
Max Wage In 10 Years Before Report	375,601	360,899	410,516	499,895	362,134
Panel D: Family Demographics					
With Child Under 12	49.2%	44.9%	53.4%	53.0%	53.3%
If Yes, How Many?	1.63	1.60	1.65	1.60	1.70
Unmarried	10.6%	11.4%	10.7%	6.6%	7.8%
Married	69.9%	65.0%	73.6%	80.9%	73.5%
Divorced	18.6%	22.5%	15.0%	12.2%	17.8%
Panel E: Employment Status					
With Stable Employer	45.0%	43.9%	46.5%	46.9%	41.3%
Public Worker	2.3%	2.1%	2.5%	2.7%	2.4%
Generic Private Worker	42.7%	41.8%	44.0%	44.1%	38.9%
Mean Wage	29,724	29,525	29,960	29,950	29,397
Max Wage In 10 Years Before Report	311,838	305,113	321,527	319,183	290,360
Panel F: IPV Duration					
Survey Rate	63.0%	74.9%	56.4%	23.8%	50.9%
If Surveyed, Duration = 0	11.6%	10.9%	14.1%	5.1%	4.0%
If Surveyed, Duration >= 10	16.5%	14.7%	17.9%	26.0%	23.9%
If Surveyed, Duration > 0 & < 10	71.9%	74.4%	67.9%	69.0%	72.2%
Mean Duration If Duration > 0 & < 10	2.76	2.56	3.05	3.18	3.24
Observations	90,604	44,784	35,355	6,362	4,103

Notes: This table builds on all female once-reporters and further breaks them down by report channels. All wages are measured in NTD per year.

Table B.6: Multinomial Logit Regression Across Report Channels

	Marginal Effect Among Report Channels				
	Hospital	*Police*	*Helpline*	*Other*	*Observation*
Panel A: Victim Information					
Report Age	-0.004	0.002	0.001	0.001	90,604
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	
NO. Children Under 12	0.016	-0.031	0.008	0.008	90,604
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	
Education year	0.023	-0.032	0.012	-0.003	90,604
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	
Income Quartile	0.014	-0.009	-0.001	-0.004	90,604
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	
Panel B: Perpetrator Information					
Perpetrator Education year	0.024	-0.026	0.005	-0.002	65,813
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	
Perpetrator Income Quartile	0.023	-0.026	0.005	-0.002	65,813
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	

Notes: This table presents how different contributing factors affect reporting channel decisions. The coefficients are marginal effects from a multinomial logit regression. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table B.7: Heterogeneity in Report Channel

Sample	Average Violence Effect ($t \geq$ -1) *DiD Coef.*	*Mean in Period -2*	Average Report Effect ($t \geq$ -1) *DiD Coef.*	*Mean in Period -2*
Panel A: Employment Rate				
Police	-0.002	0.581	0.001	0.592
	(0.004)		(0.003)	
Hospital	-0.006	0.609	0.006*	0.644
	(0.007)		(0.003)	
Helpline	0.006	0.626	0.019**	0.677
	(0.027)		(0.008)	
Panel B: Get Divorced Dummy				
Police	0.033***	0.024	0.035***	0.017
	(0.002)		(0.001)	
Hospital	0.026***	0.021	0.040***	0.024
	(0.002)		(0.001)	
Helpline	0.025***	0.018	0.053***	0.015
	(0.009)		(0.003)	
Panel C: Fertility Rate				
Police	-0.003	0.089	-0.022***	0.065
	(0.002)		(0.002)	
Hospital	0.001	0.058	-0.041***	0.052
	(0.004)		(0.002)	
Helpline	0.010	0.071	-0.043***	0.077
	(0.017)		(0.005)	
Panel D: Depression Visit Dummy				
Police	0.009***	0.061	-0.002	0.100
	(0.003)		(0.002)	
Hospital	0.010**	0.065	-0.003	0.081
	(0.004)		(0.002)	
Helpline	0.009	0.063	0.000	0.079
	(0.013)		(0.006)	
Panel E: Regression Information				
Police Unique *ID NO.*	24,958		44,784	
Regression Observation	3,246,978		3,841,814	
Hospital Unique *ID NO.*	13,548		35,355	
Regression Observation	1,571,174		2,985,120	
Helpline Unique *ID NO.*	1,044		6,362	
Regression Observation	72,528		604,582	

Notes: This table investigates heterogeneity across report channels. Instead of a dynamic framework, a simple static framework is presented here where only before-after estimates are shown. The mean in period -2 is also presented. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table B.8: IPV Duration Analysis

Dependent Variable: Duration Year	*Baseline OLS* (1) *mean = 2.76*	(1) + Income Covariates (2) *mean = 2.76*	(2) + Channel Dummy (3) *mean = 2.76*	(3) + Perpetrator Characteristics (4) *mean = 2.72*
Reporting Age	0.027***	0.027***	0.029***	0.024***
	(0.002)	(0.002)	(0.002)	(0.002)
Education Year	-0.003	0.000	-0.006	-0.0120**
	(0.004)	(0.004)	(0.004)	(0.005)
NO. Children Under 12	0.278***	0.278***	0.260***	0.237***
	(0.012)	(0.012)	(0.012)	(0.013)
Income Percentile 26-50		-0.126**	-0.128**	-0.131**
		(0.052)	(0.051)	(0.059)
Income Percentile 51-75		-0.102*	-0.113*	-0.145**
		(0.060)	(0.060)	(0.068)
Income Percentile 76-100		-0.189***	-0.197**	-0.221***
		(0.061)	(0.061)	(0.069)
Work Dummy (1 Year Before Report)		0.127**	0.131**	0.169***
		(0.054)	(0.054)	(0.061)
Report Through Other Channel			0.150***	0.082
			(0.054)	(0.064)
Report Through Police			-0.493***	-0.564***
			(0.022)	(0.028)
Report Through Helpline			0.108*	0.041
			(0.064)	(0.079)
Perpetrator Education Year				0.001
				(0.005)
Perpetrator Income Percentile 26-50				0.050
				(0.032)
Perpetrator Income Percentile 51-75				0.045
				(0.032)
Perpetrator Income Percentile 76-100				0.052
				(0.034)
Observations	41,057	41,057	41,057	32,323

Notes: This table investigates contributing factors in IPV duration year. The outcome variable, *Duration Year*, is defined as the year between violence onset and report. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table B.9: Summary Statistics by IPV Survey Status

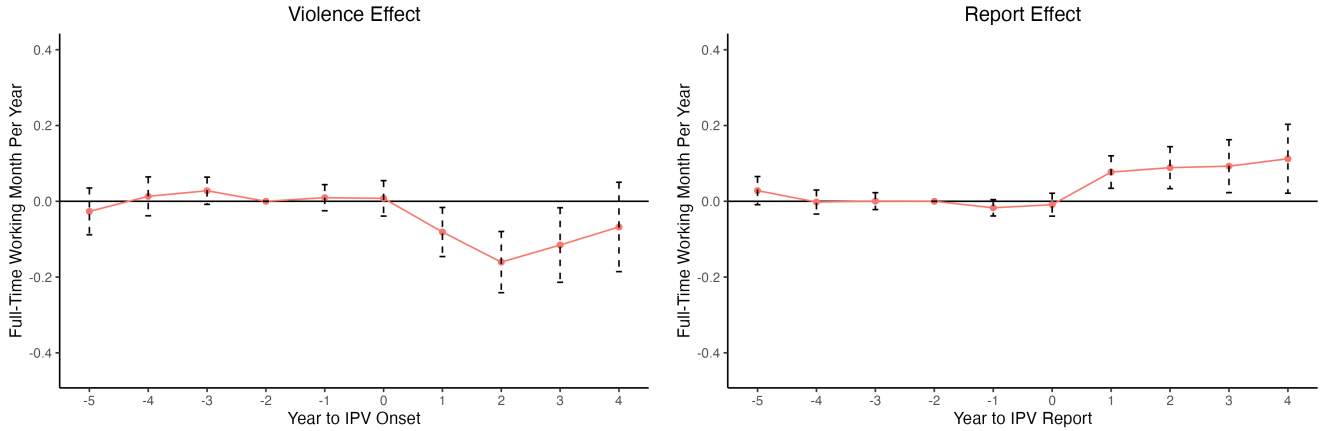
Sample: Female Once-reporters Age 25-55	All (1)	Without Survey (2)	With Survey (3)
Panel A: Victim statistics			
Mean Age	39.61	39.54	39.65
Mean Education Years	12.27	12.29	12.25
Depression Outpatient-visit	9.6%	9.7%	9.6%
Panel B: Social Worker Evaluation			
Labeled "Severe" & Provided Assistance	33.1%	-	33.1%
IPV Severity Index (Out of 13)	3.66	-	3.66
Panel C: Perpetrator Statistics			
Mean Age	42.84	42.88	42.82
Mean Education Years	11.72	11.78	11.68
Max Wage In 10 Years Before Report	375,601	382,534	371,196
Panel D: Family Demographics			
With Child Under 12	49.2%	49.82%	48.78%
If Yes, How Many?	1.63	1.64	1.62
Unmarried	10.6%	10.9%	10.4%
Married	69.9%	70.6%	69.5%
Divorced	18.6%	17.7%	19.2%
Panel E: Employment Status			
With Stable Employer	45.0%	45.1%	45.0%
Public Worker	2.3%	2.4%	2.3%
Generic Private Worker	42.7%	42.7%	42.7%
Mean Wage (NTD/month)	29,724	29,935	29,601
Max Wage In 10 Years Before Report	311,838	313,252	311,007
Panel F: Reporting Behavior			
Source: Police	49.4%	33.6%	58.7%
Source: Hospital	39.0%	46.0%	34.9%
Source: Helpline	7.0%	14.5%	2.7%
Source: Other	4.6%	5.9%	3.7%
Observations	90,604	33,529	57,075

Notes: This table decomposes all 90,604 female once-reporters by survey status. All wages are measured in NTD per year.

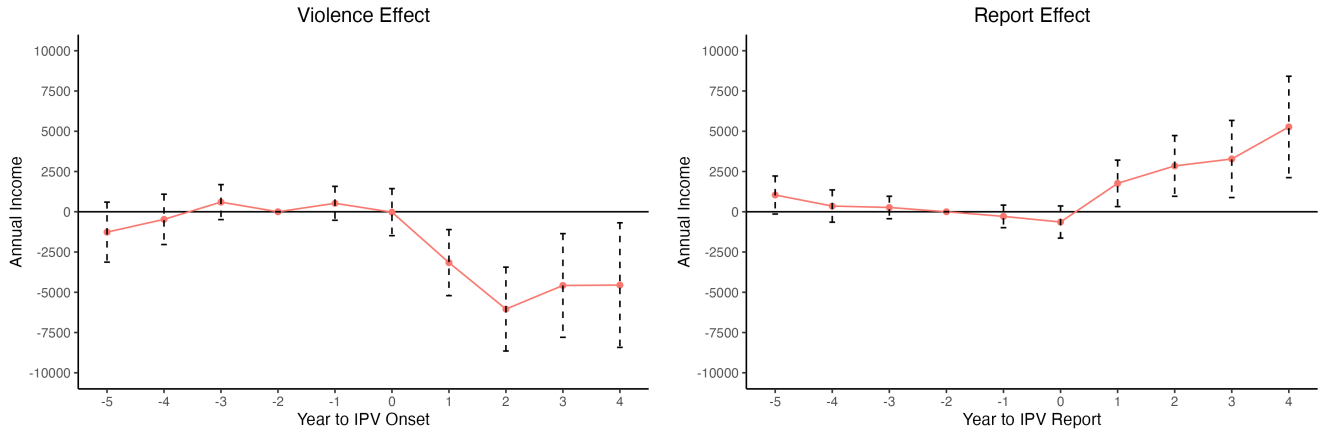
C Online Appendix - Figure

This is the online appendix with extra details.

Panel A: Working Month Per Year



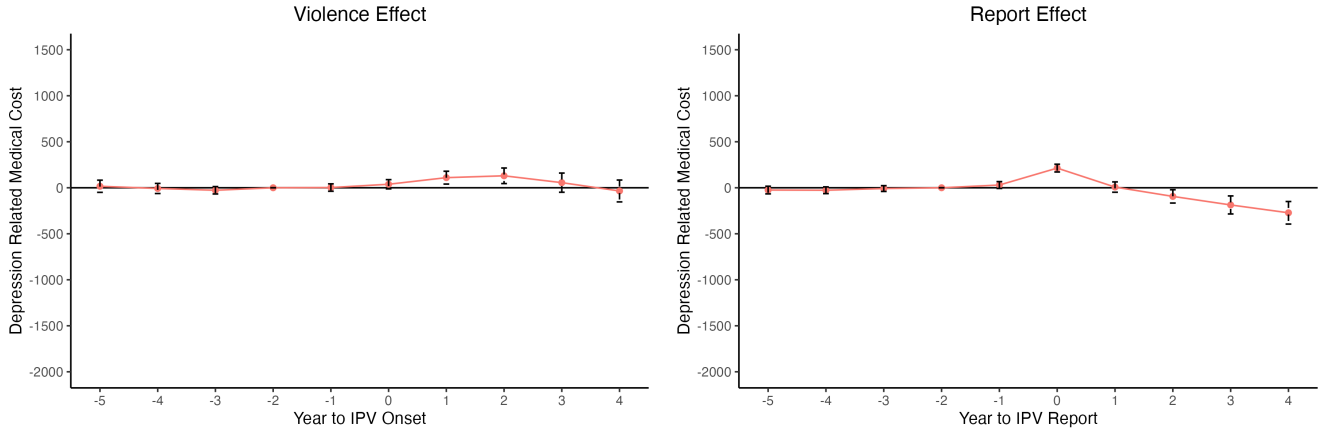
Panel B: Annual Income



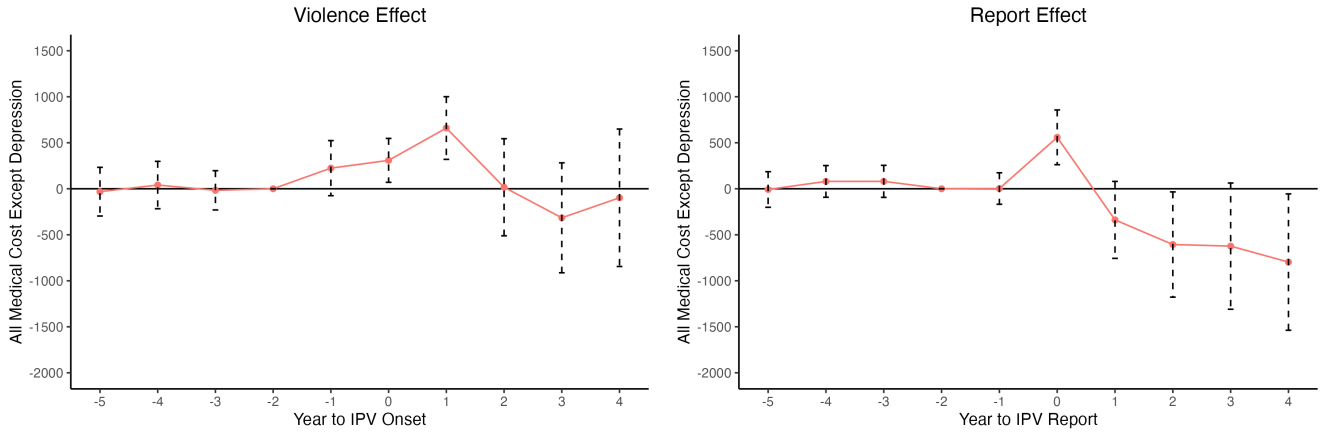
Notes: This figure investigates other labor market performance in addition to our main outcome - annual employment rate. Dash lines are 95% confidence interval of the estimates.

Figure C.1: Other Labor Market Performances

Panel A: Depression Outpatient Expense



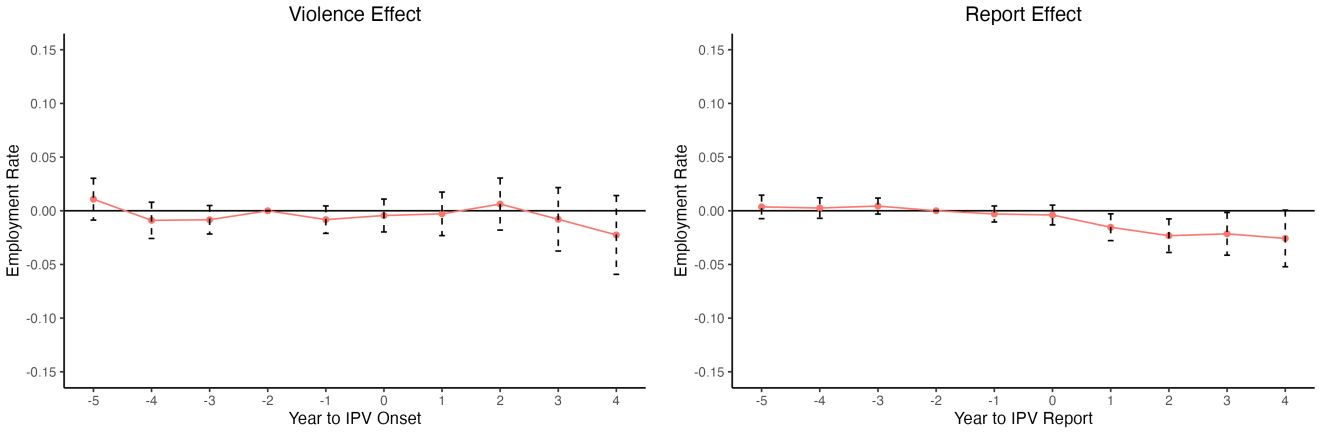
Panel B: All Outpatient Expense (Except Depression)



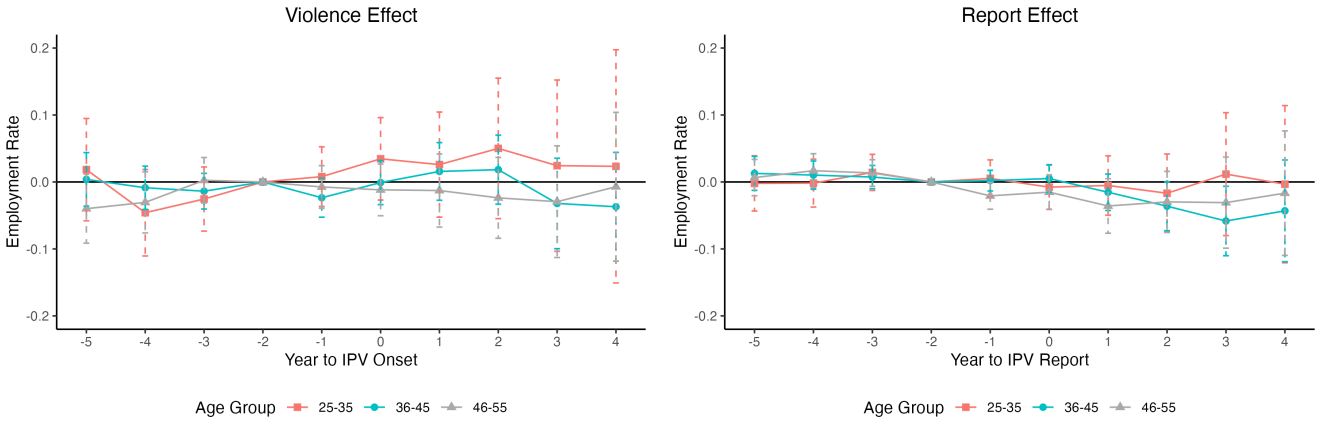
Notes: This figure investigates other health outcomes in addition to our main outcome - depression outpatient visit. Dash lines are 95% confidence interval of the estimates.

Figure C.2: Other Health Outcomes

Panel A: Overall Effect



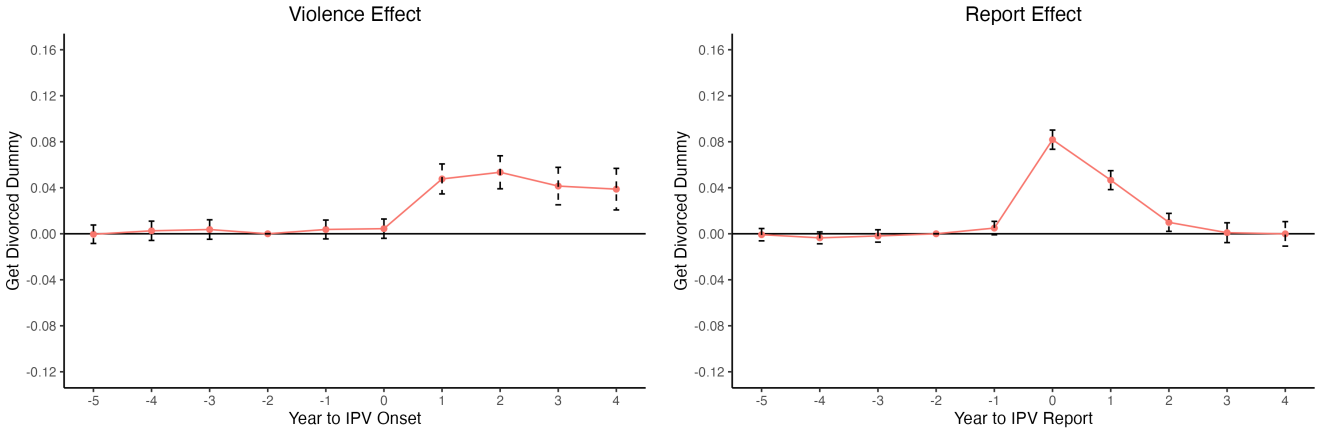
Panel B: Reporting Age Breakdown



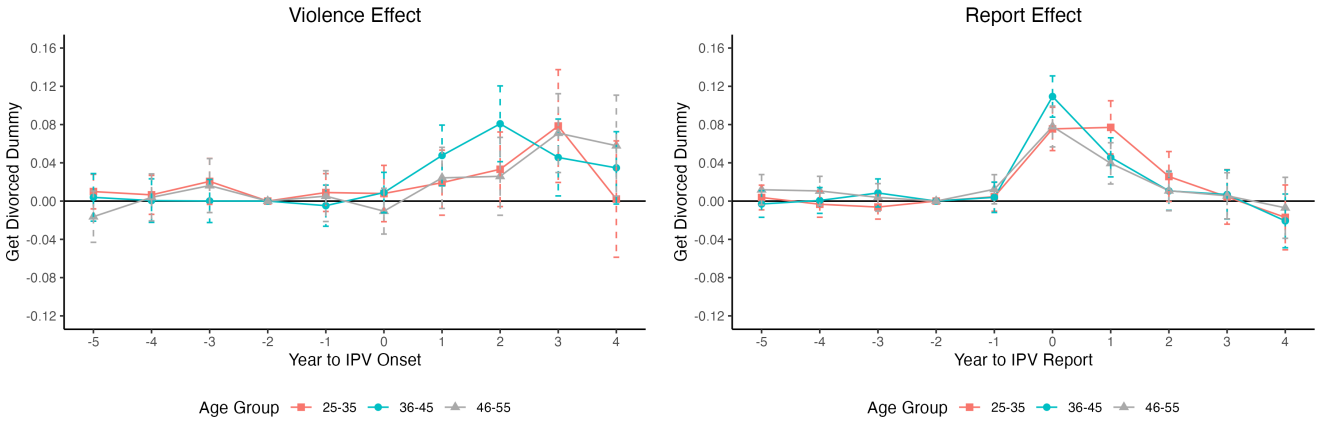
Notes: Panel A presents dynamic event-study coefficients for both violence and report effects, which are then decomposed into 3 age groups in Panel B. Dash lines are 95% confidence interval of the estimates. The outcome variable, *Employment Rate*, is defined as 1 if the victim works more than 6 months in a given year, and 0 otherwise.

Figure C.3: Employment Rate Among Male Once-reporters

Panel A: Overall Effect



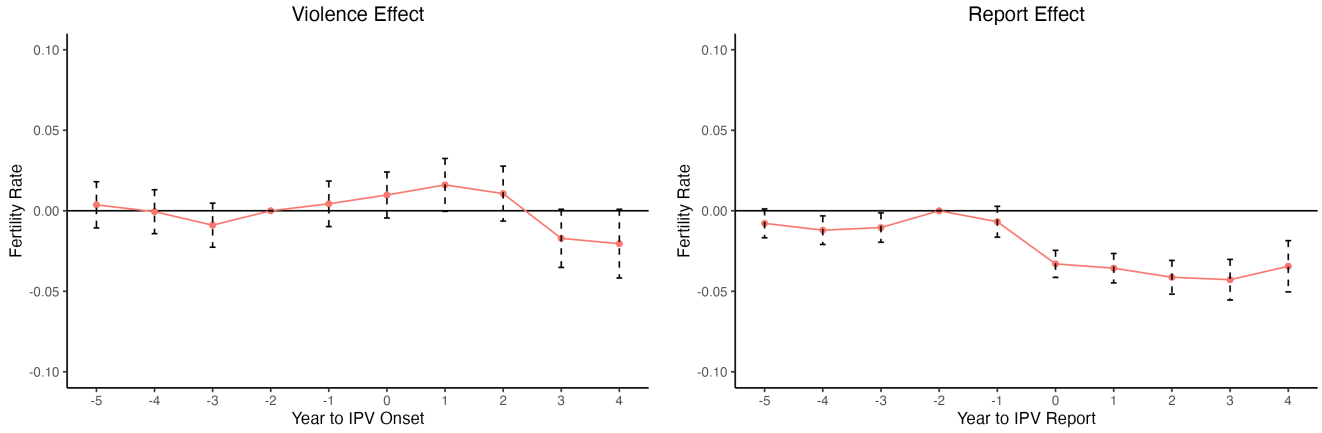
Panel B: Reporting Age Breakdown



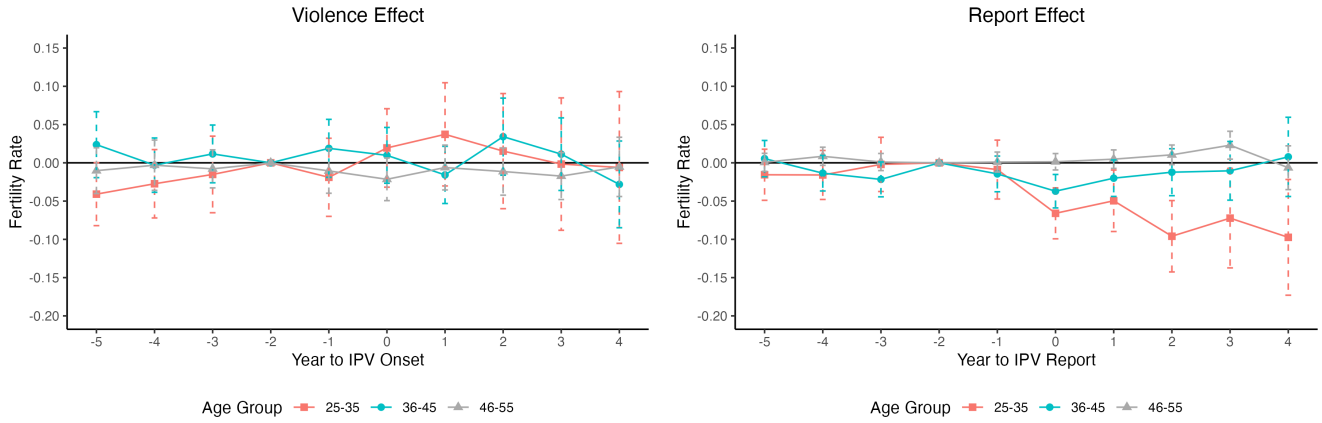
Notes: Panel A presents dynamic event-study coefficients for both violence and report effects, which are then decomposed into 3 age groups in Panel B. Dash lines are 95% confidence interval of the estimates. The outcome variable, *Get-divorced Dummy*, is defined as 1 if the victim gets divorced in a given year, and 0 otherwise.

Figure C.4: Divorce Decision Among Male Once-reporters

Panel A: Overall Effect



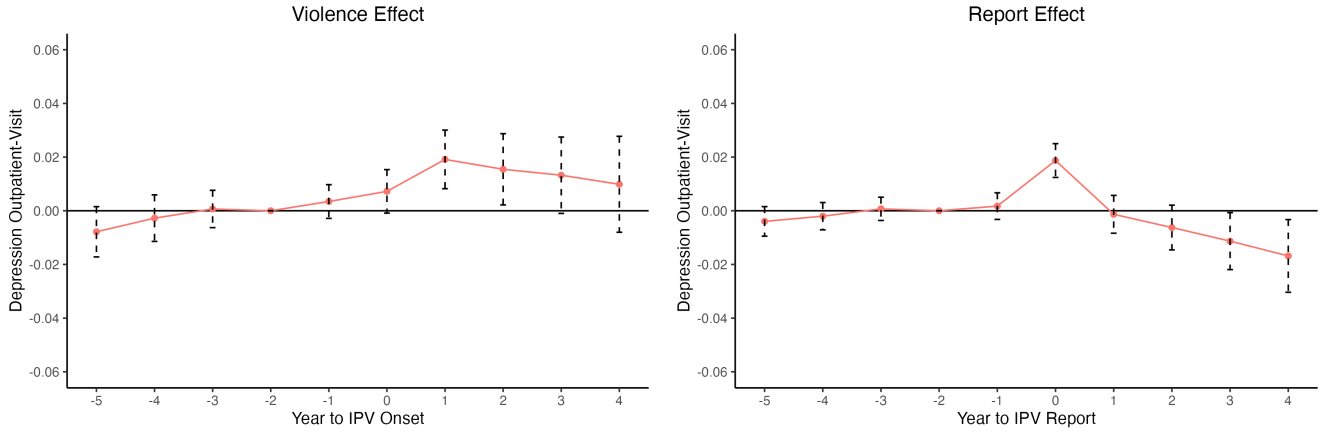
Panel B: Reporting Age Breakdown



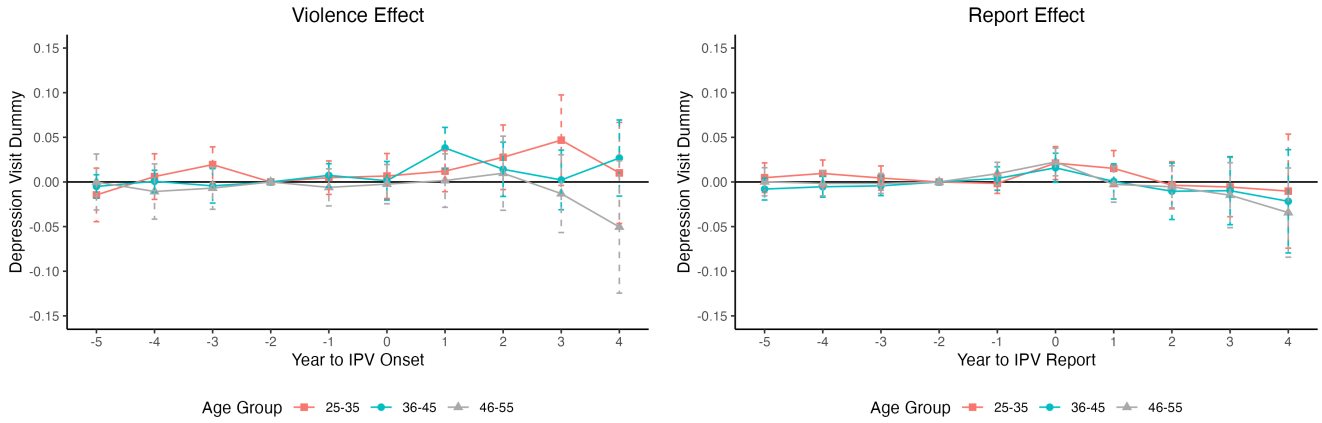
Notes: Panel A presents dynamic event-study coefficients for both violence and report effects, which are then decomposed into 3 age groups in Panel B. Dash lines are 95% confidence interval of the estimates. The outcome variable, *Give-birth Dummy*, is defined as 1 if the victim becomes a father in a given year, and 0 otherwise.

Figure C.5: Fertility Decision Among Male Once-reporters

Panel A: Overall Effect



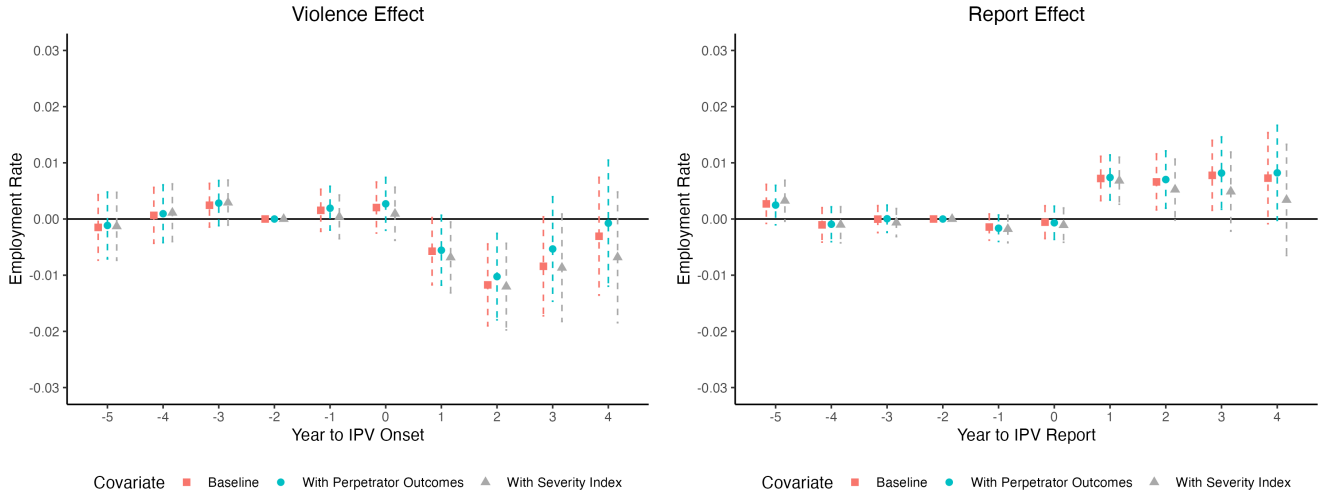
Panel B: Reporting Age Breakdown



Notes: Panel A presents dynamic event-study coefficients for both violence and report effects, which are then decomposed into 3 age groups in Panel B. Dash lines are 95% confidence interval of the estimates. The outcome variable, *Depression Visit Dummy*, is defined as 1 if the victim visits a depression outpatient clinic in a given year, and 0 otherwise.

Figure C.6: Depression Outpatient Visit Among Male Once-reporters

Panel A: Employment Rate



Panel B: Divorce Decision

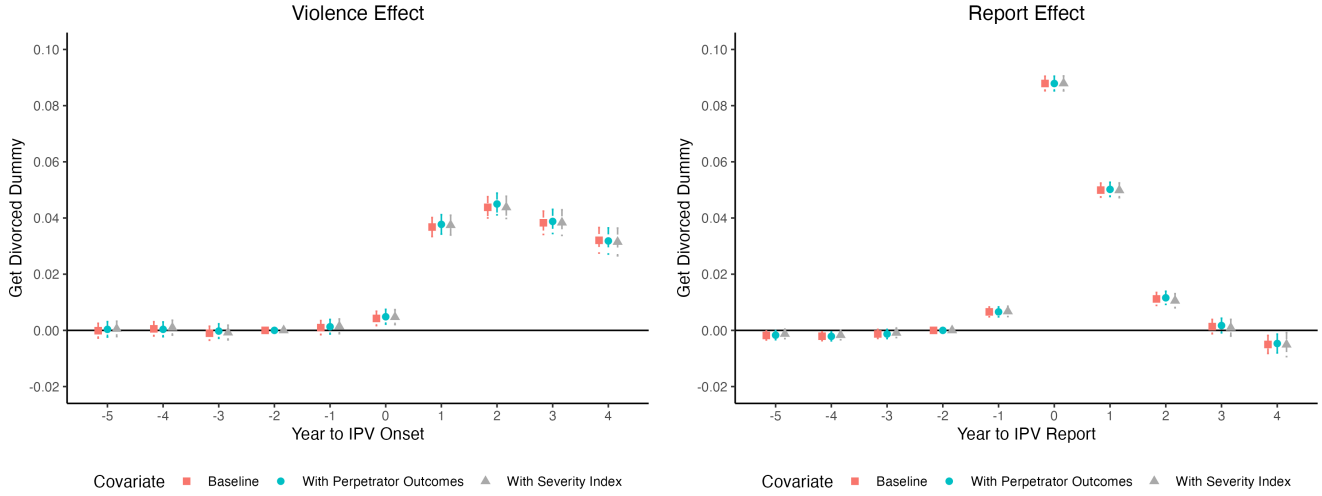
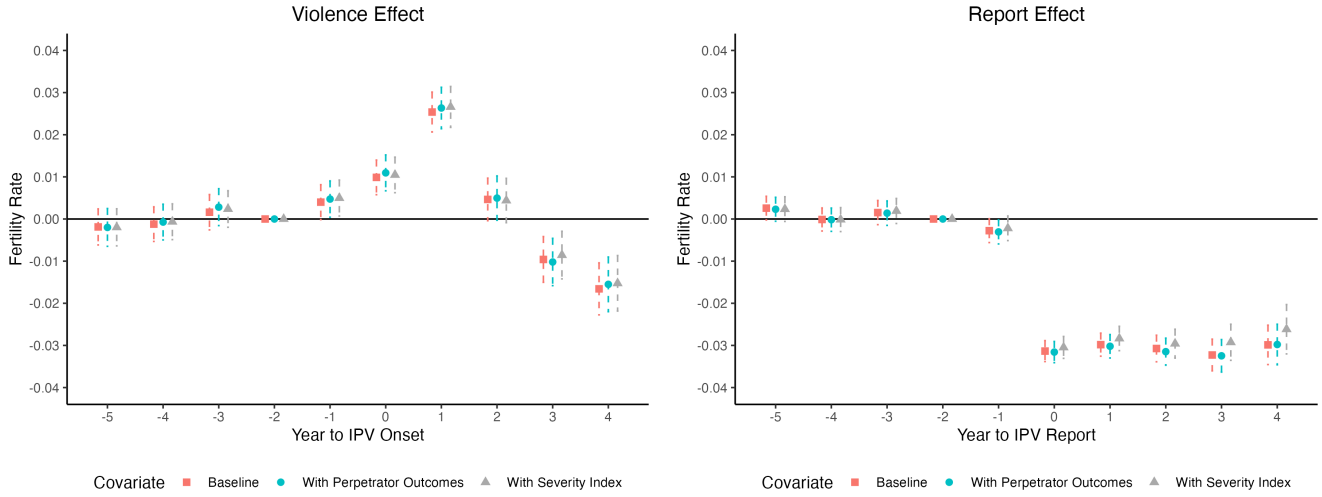
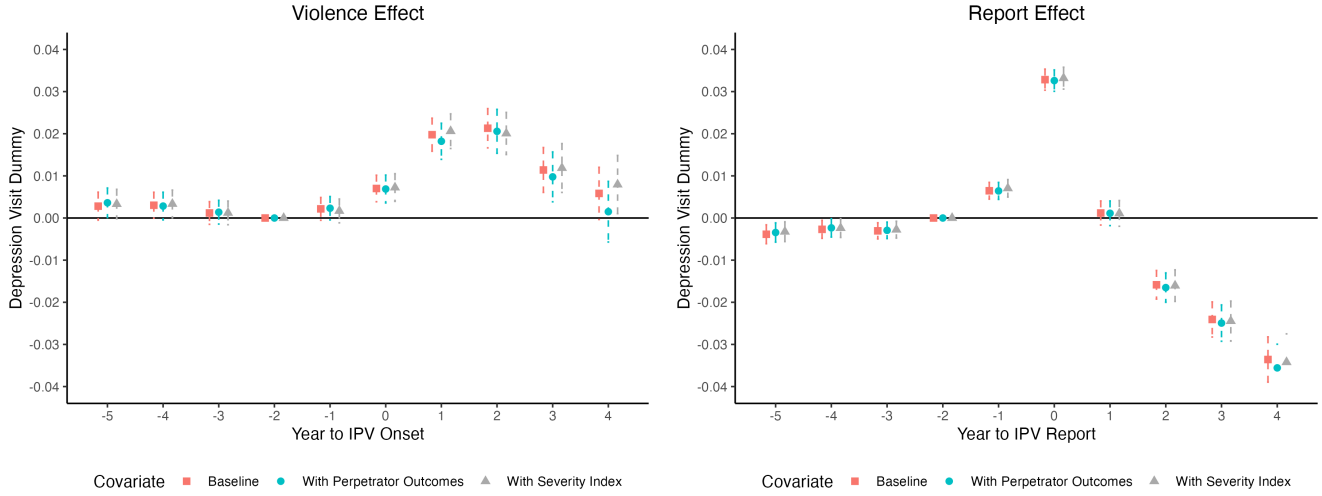


Figure C.7: Staggered DiD With Different Covariates

Panel C: Fertility Decision



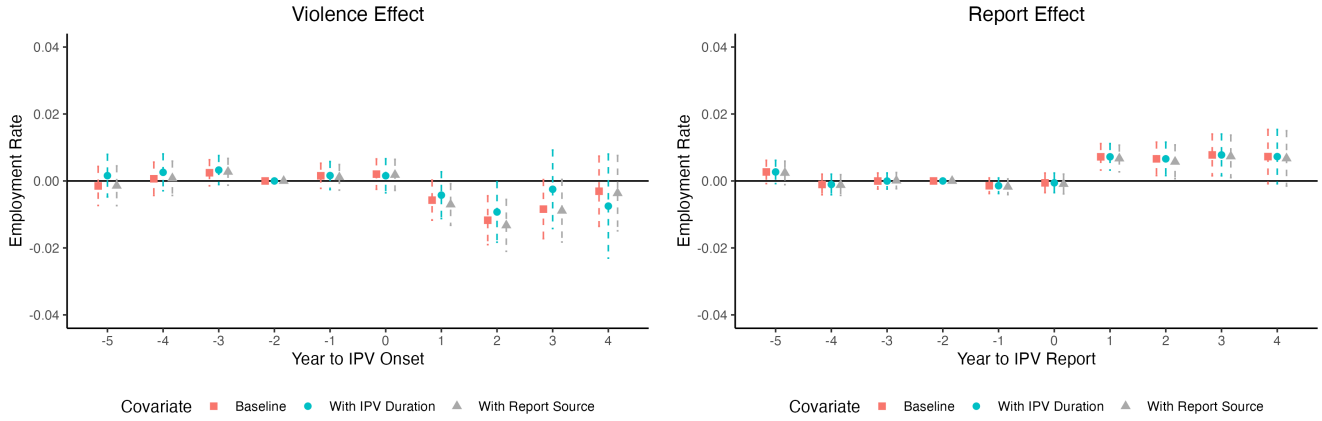
Panel D: Depression Outpatient Visit



Notes: This figure compares different covariates incorporation in the staggered DiD setting. Dash lines are 95% confidence interval of the estimates.

Figure C.7: Staggered DiD With Different Covariates (Continued)

Panel A: Employment Rate



Panel B: Divorce Decision

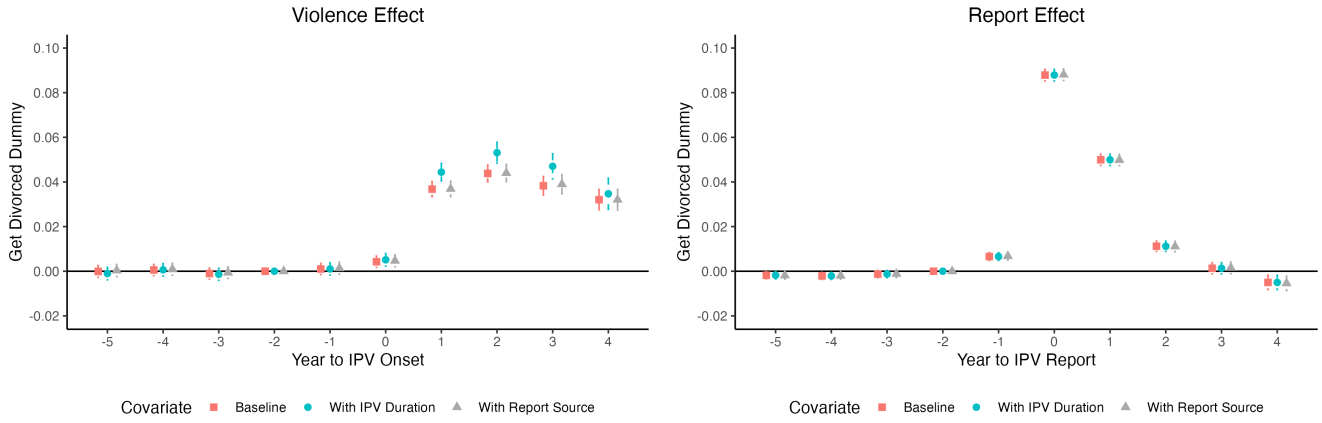
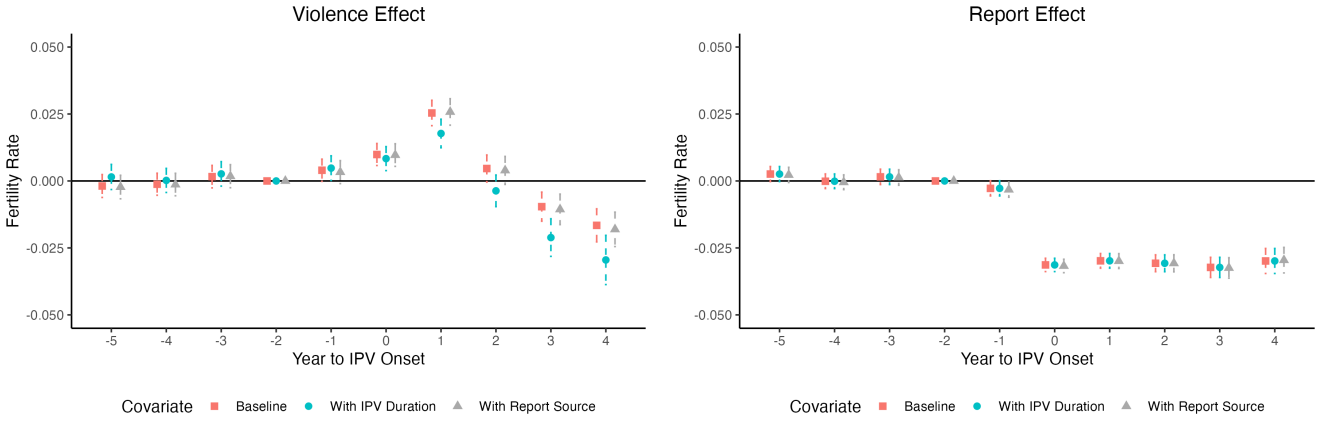
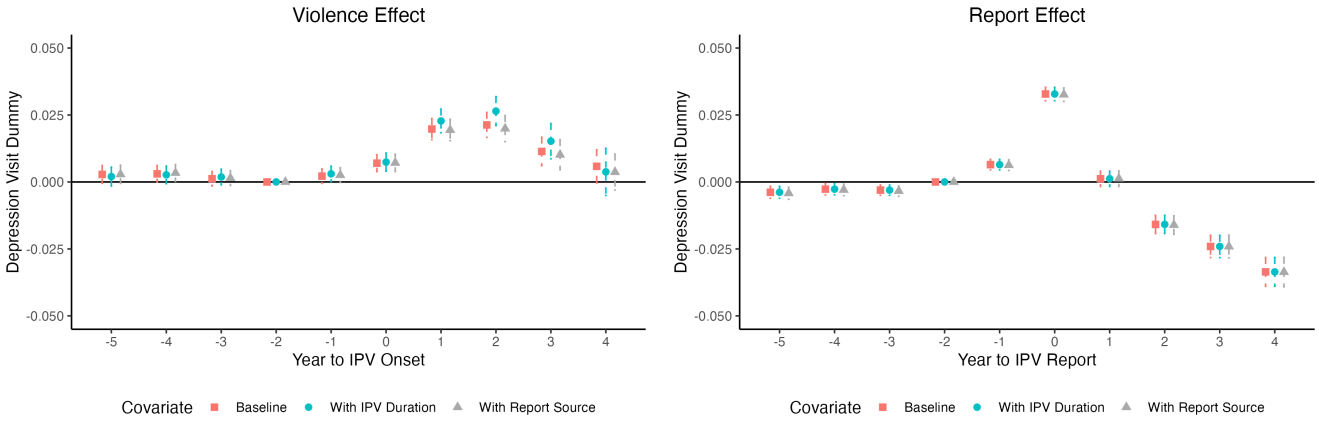


Figure C.8: Robustness Check With Covarariates Concerning Report Behavior

Panel C: Fertility Decision



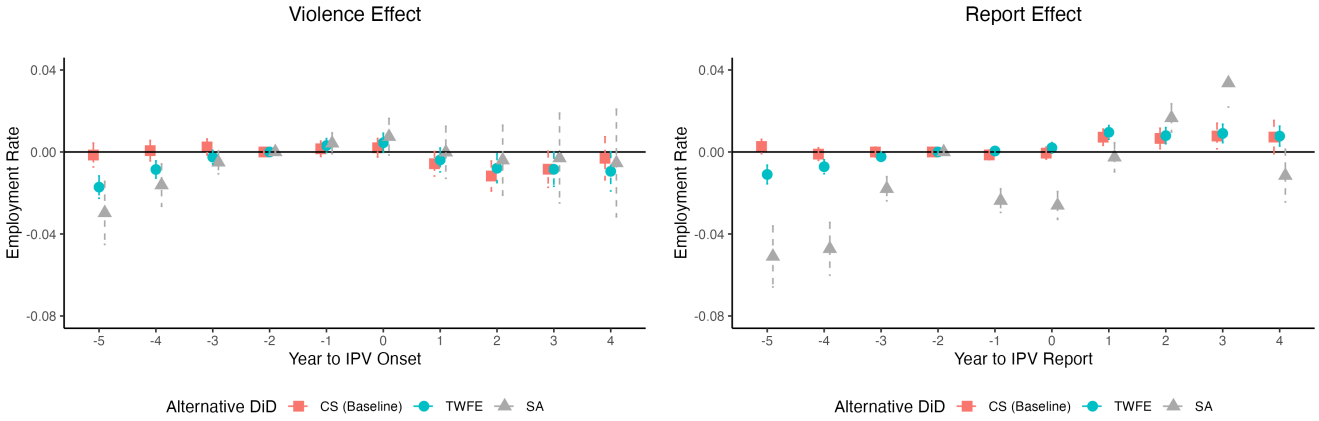
Panel D: Depression Outpatient Visit



Notes: This figure compares the baseline results to different selections of covariates that concern reporting behavior particularly. Dash lines are 95% confidence interval of the estimates.

Figure C.8: Robustness Check With Covarariates Concerning Report Behavior (Continued)

Panel A: Employment Rate



Panel B: Divorce Decision

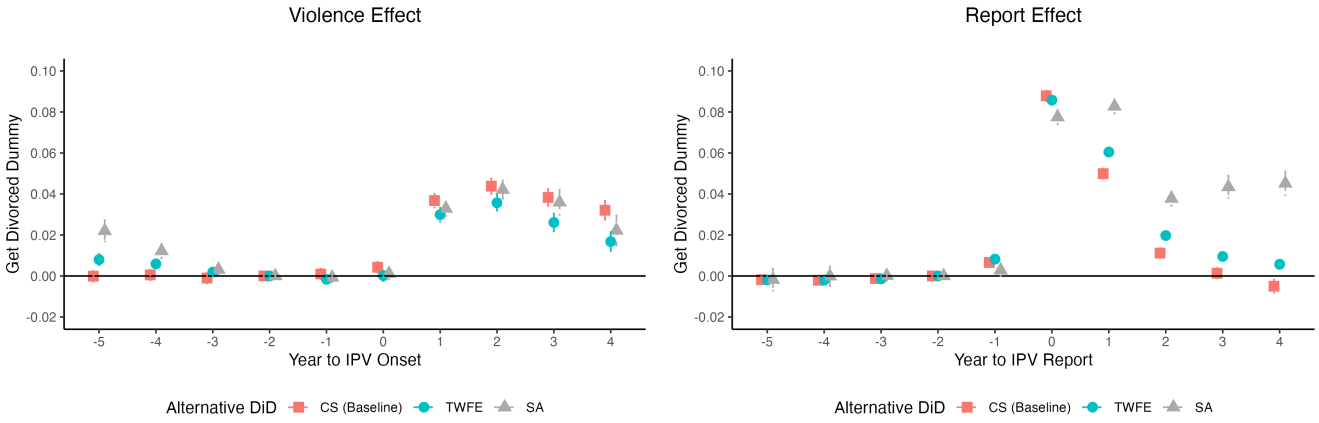
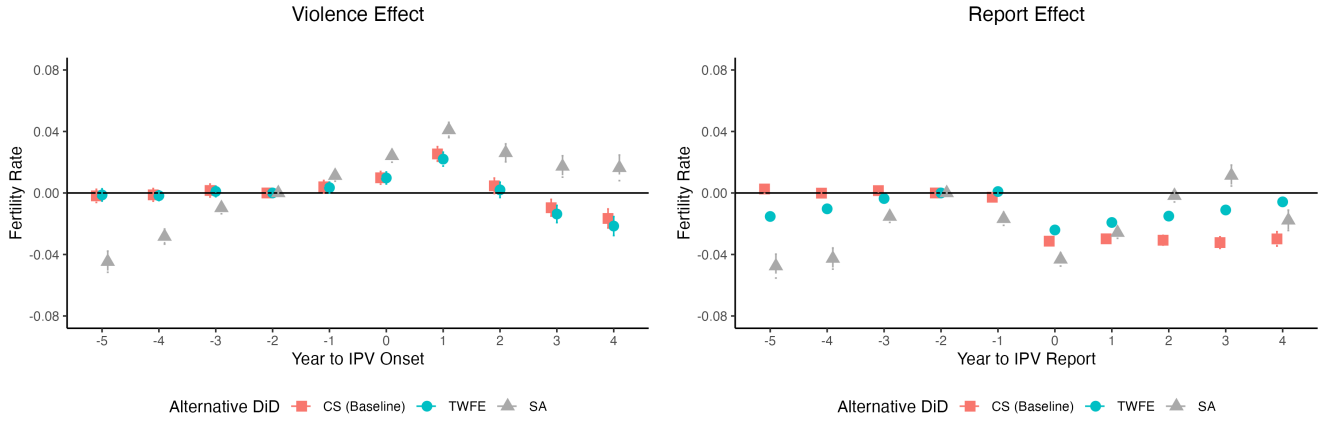
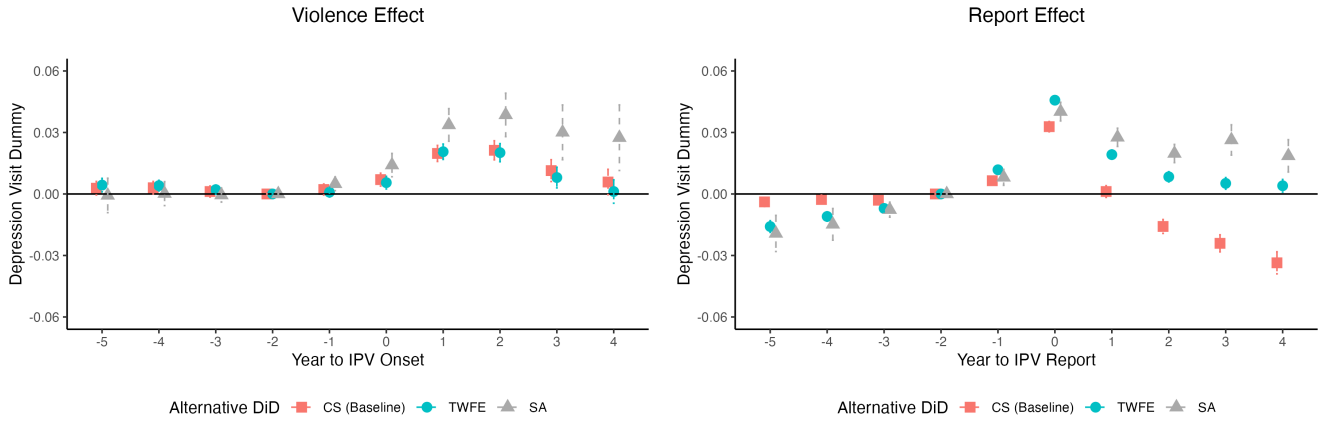


Figure C.9: Robustness Check in Alternative DiD Specification

Panel C: Fertility Decision

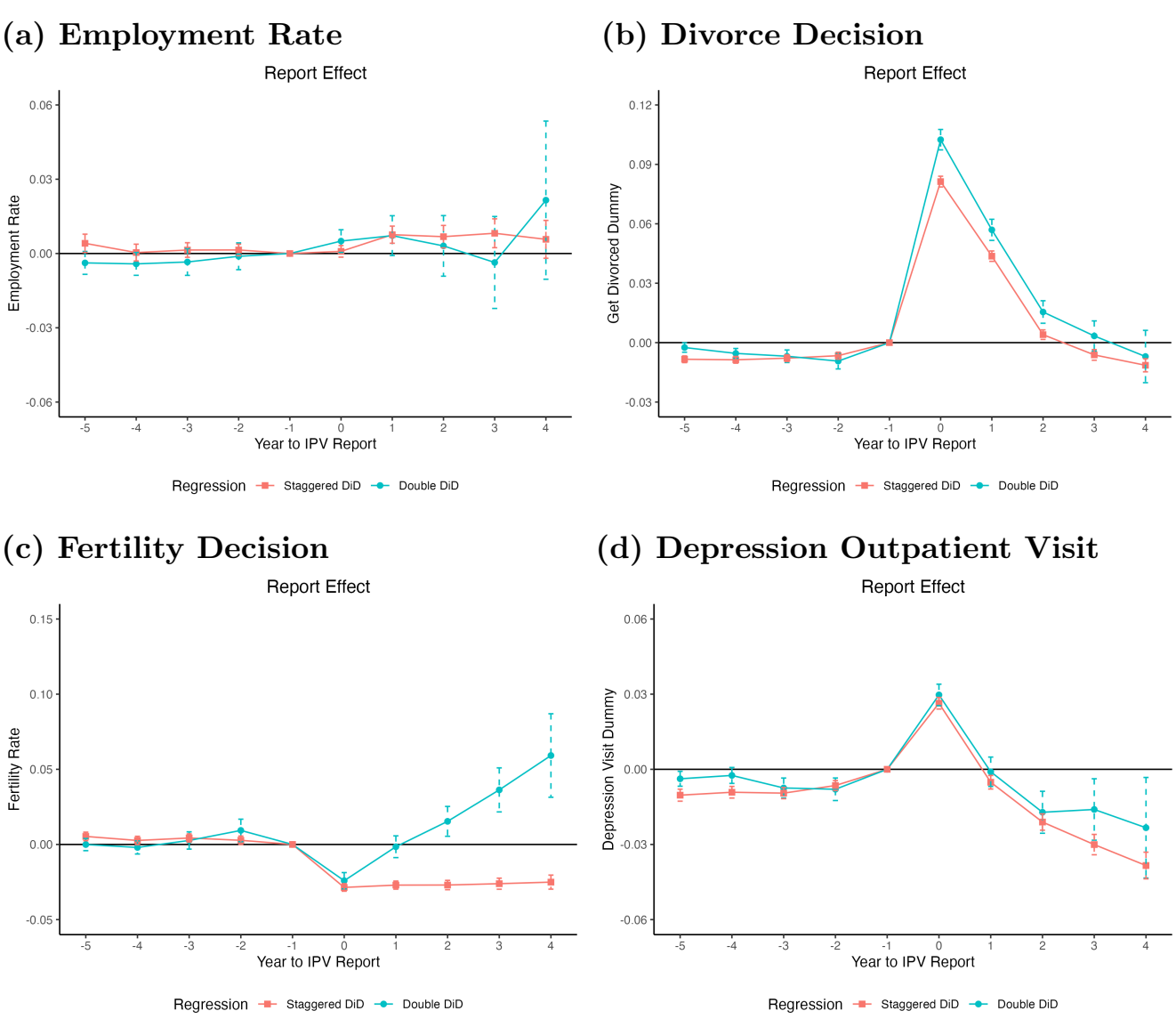


Panel D: Depression Outpatient Visit



Notes: This figure compares different DiD specifications under a staggered DiD. CS indicates Callaway and Sant'Anna (2021), which is our main empirical design. TWFE stands for Two-way Fixed Effect model; SA suggests its correction by Sun and Abraham (2021). Dash lines are 95% confidence interval of the estimates.

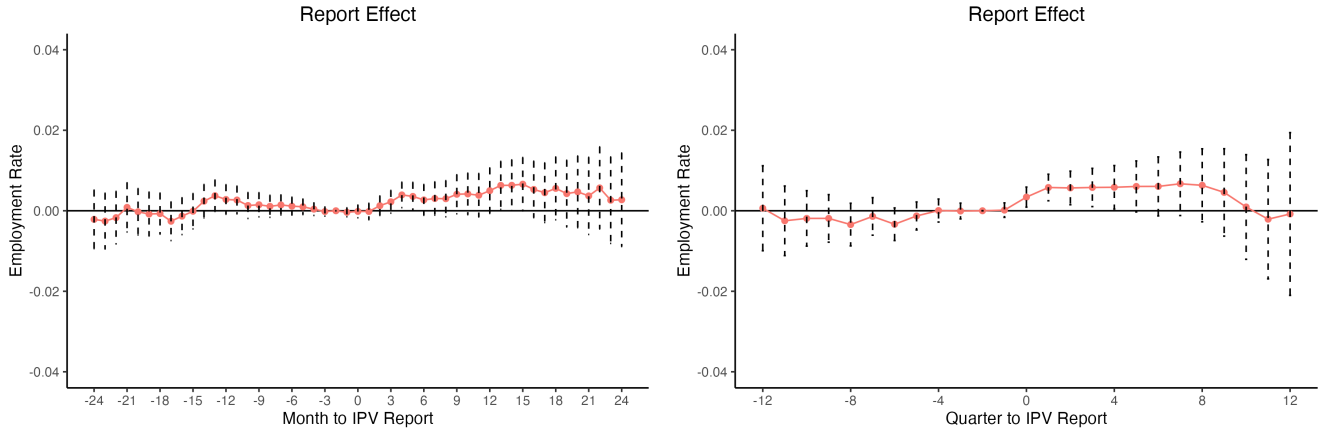
Figure C.9: Robustness Check in Alternative DiD Specification (Continued)



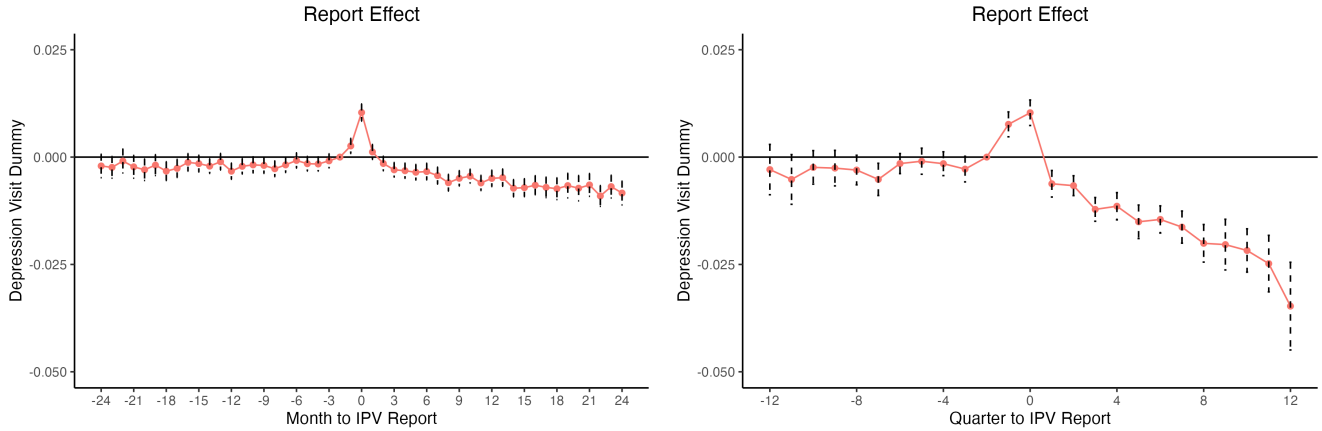
Notes: This figure estimates report effects using a double DiD method and compares to our main results. The DiD reference period is $t = -1$ instead of $t = -2$. Dash lines are 95% confidence interval of the estimates.

Figure C.10: Estimating Report Effect With Double DiD

Panel A: Employment Rate



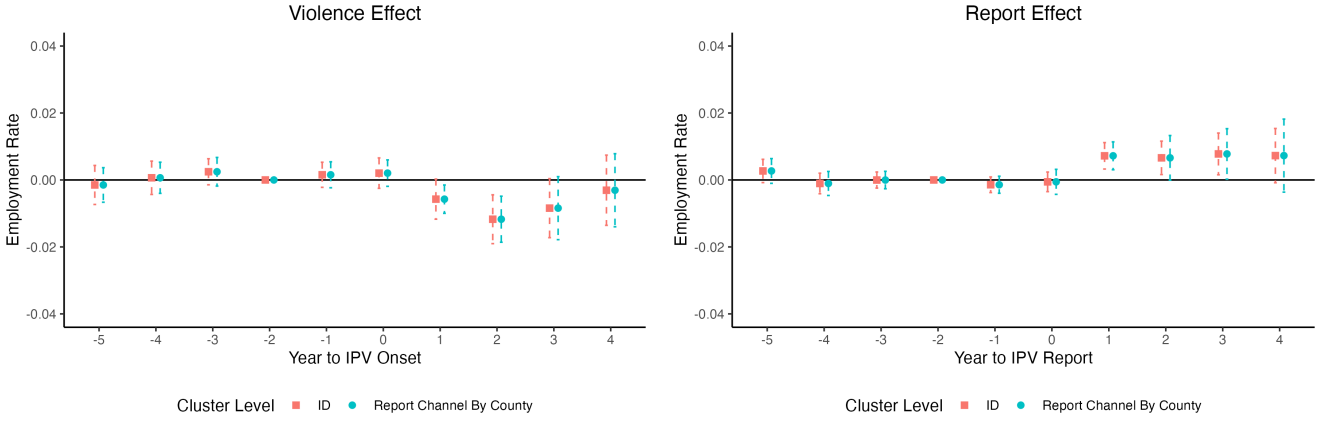
Panel B: Depression Outpatient Visit



Notes: Panels A and B expand the original yearly analysis into monthly and quarterly levels. Dash lines are 95% confidence interval of the estimates.

Figure C.11: Analysis On Monthly And Quarterly Level

Panel A: Employment Rate



Panel B: Divorce Decision

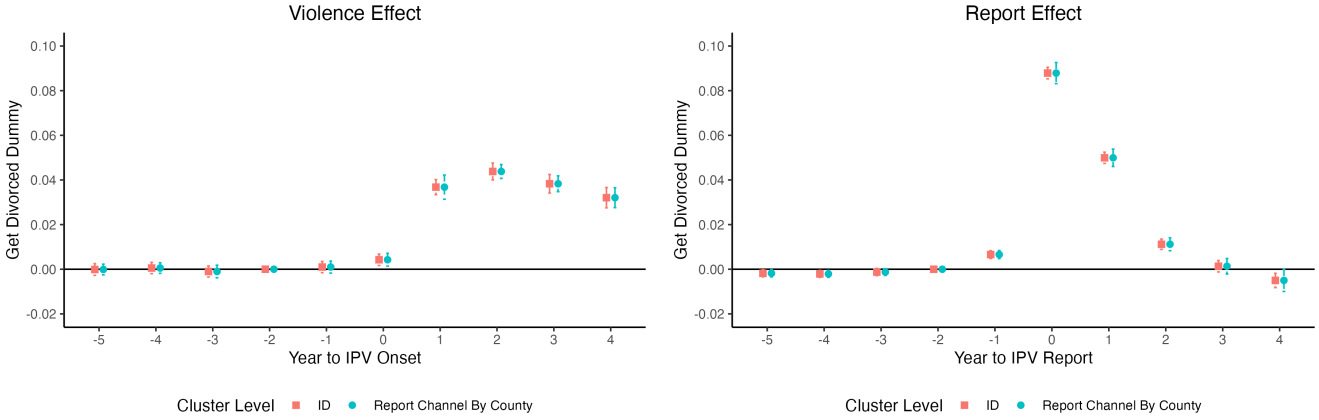
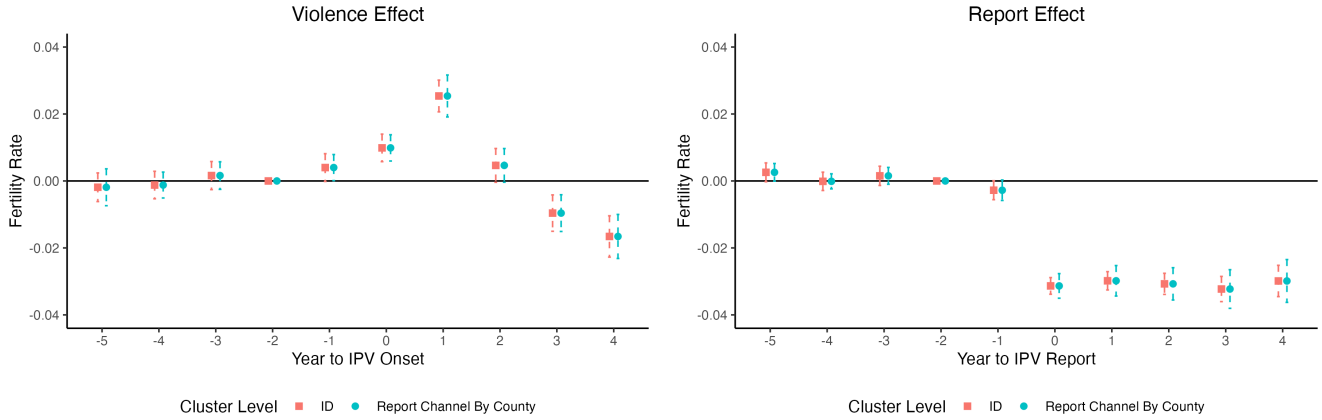
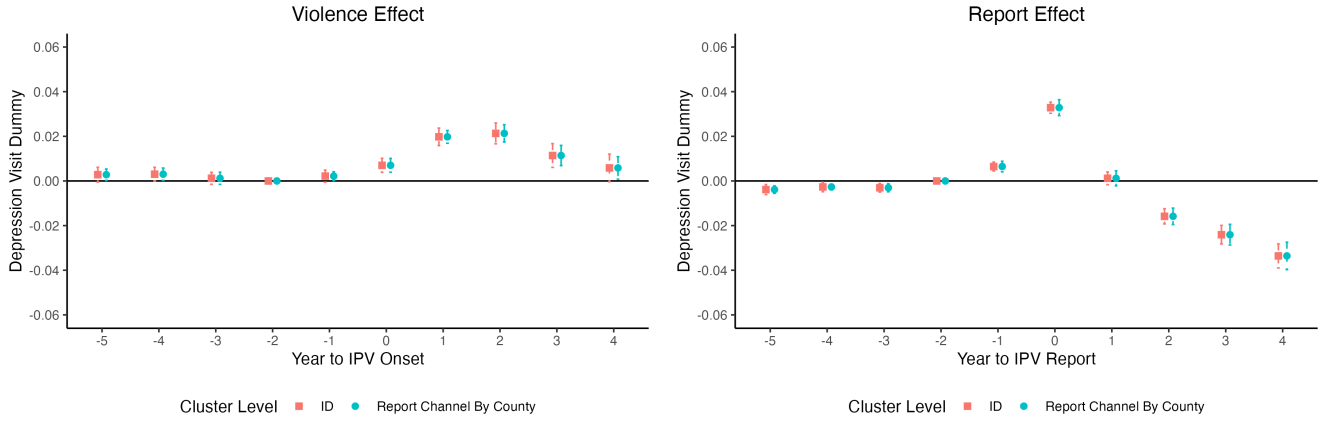


Figure C.12: Robustness Check in Staggered DiD Cluster Level

Panel C: Fertility Decision

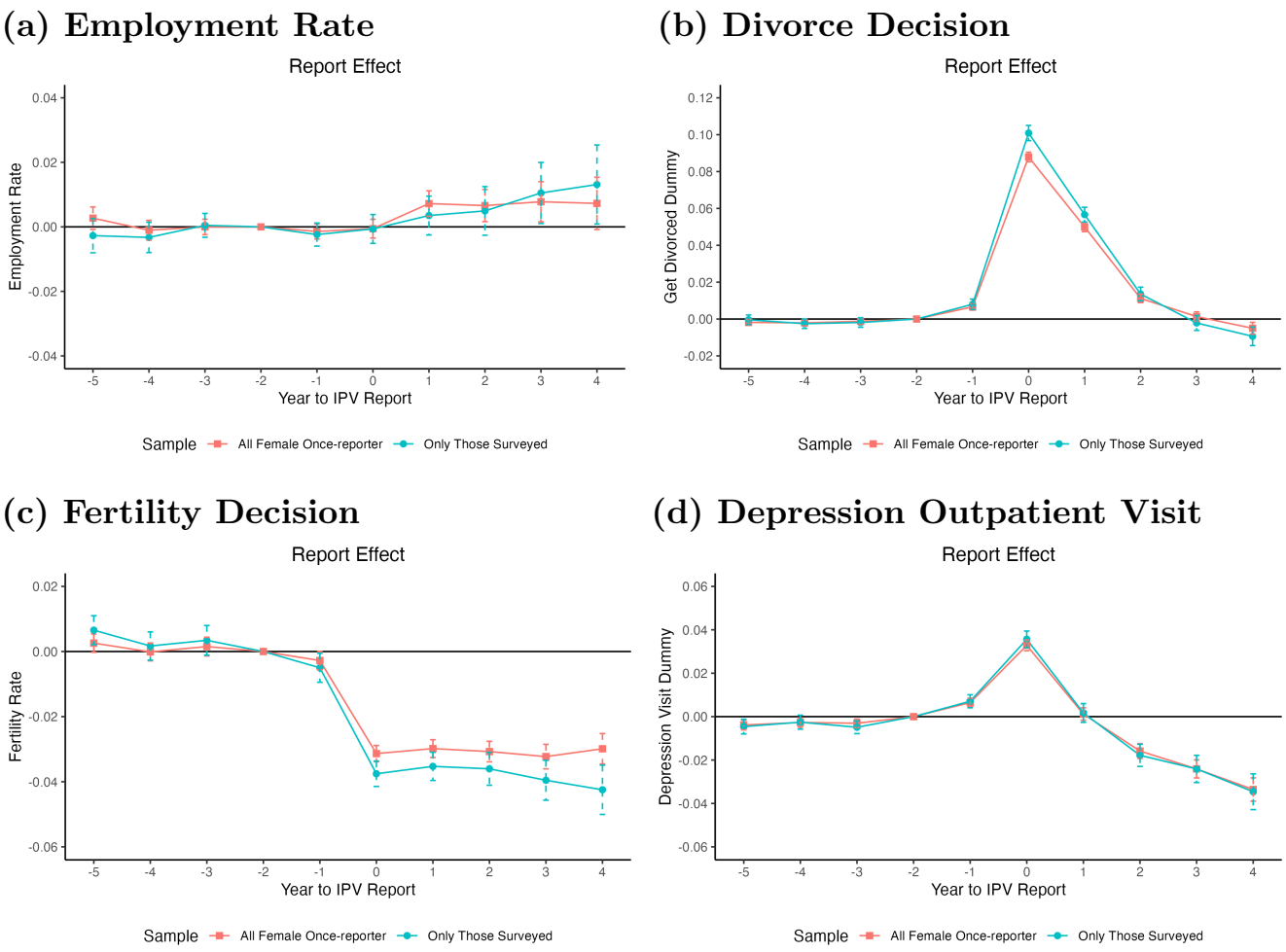


Panel D: Depression Outpatient Visit



Notes: This figure compares different cluster levels in the staggered DiD setting. Dash lines are 95% confidence interval of the estimates.

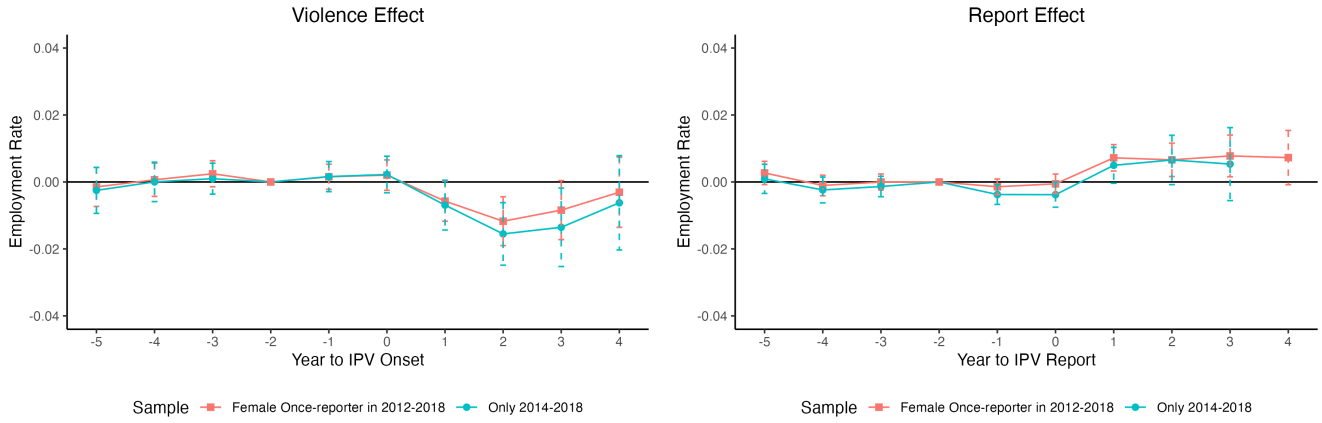
Figure C.12: Robustness Check in Staggered DiD Cluster Level (Continued)



Notes: This figure compares the report effects between our main sample (all female once-reporters) and only those who were surveyed by social workers. Dash lines are 95% confidence interval of the estimates.

Figure C.13: Report Effect by Survey Status

Panel A: Employment Rate



Panel B: Divorce Decision

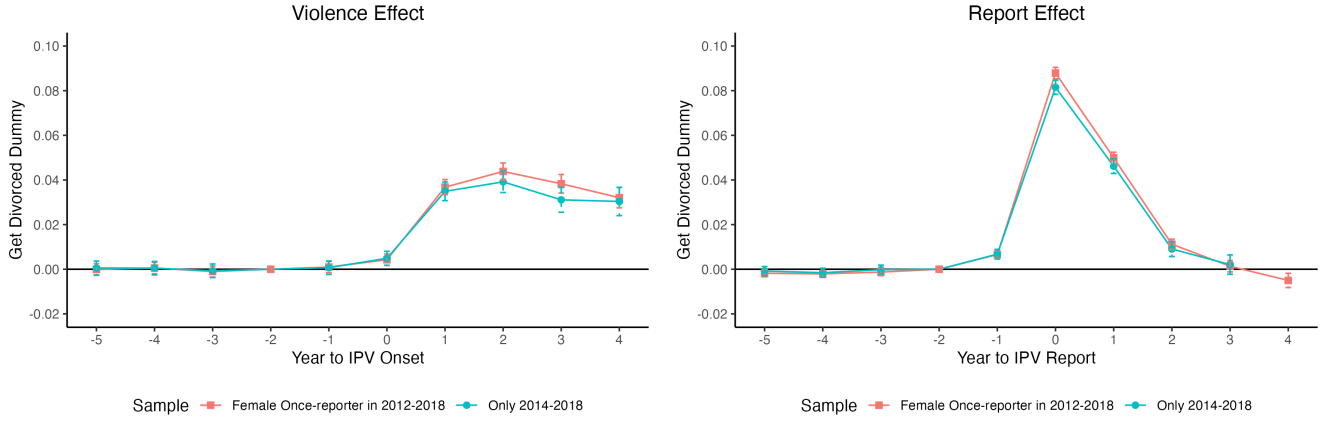
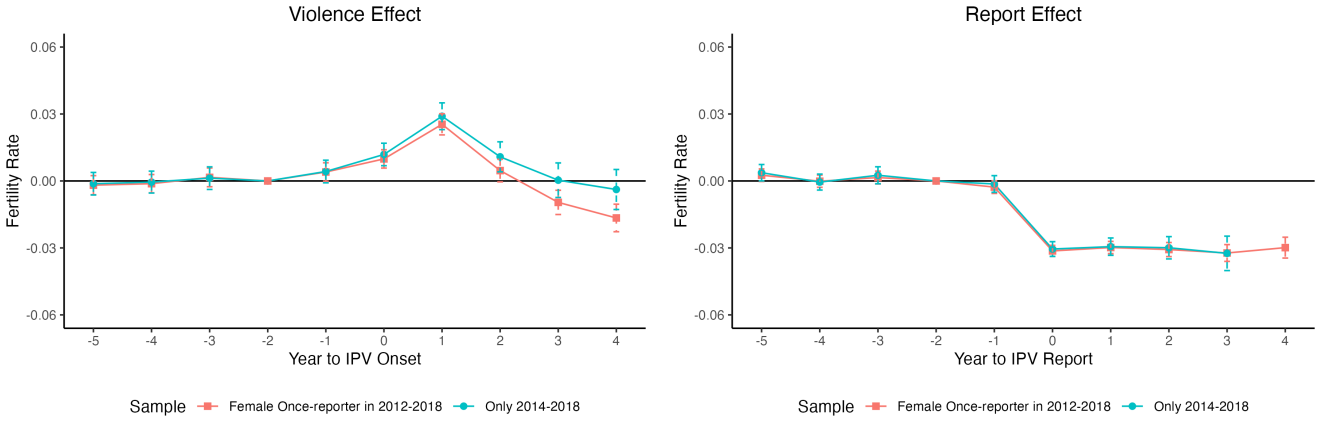
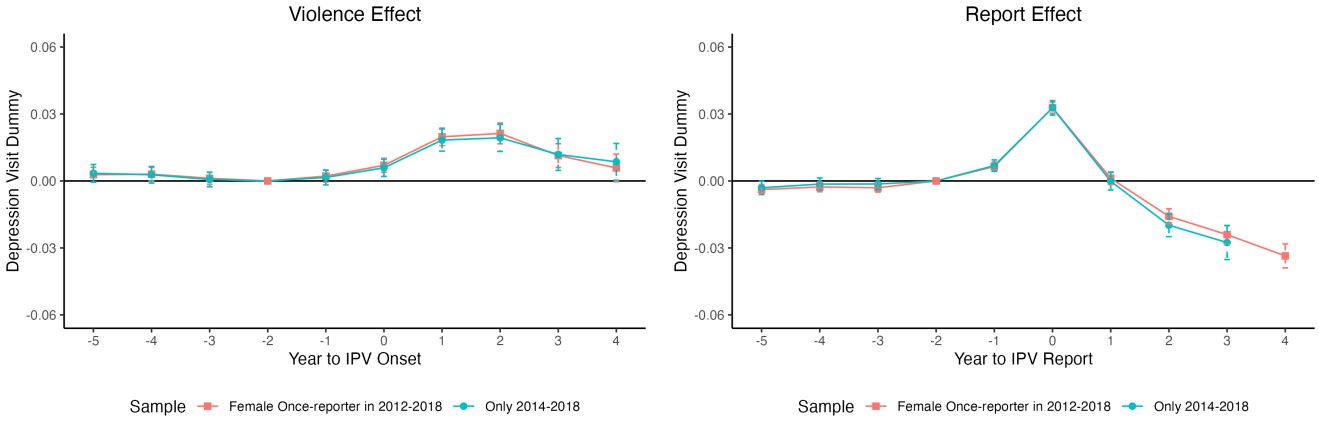


Figure C.14: Robustness Check With 2014-2018 Sample

Panel C: Fertility Decision



Panel D: Depression Outpatient Visit



Notes: This figure builds on our main result but limits the sample to all the once-reporters reporting in 2014-2018 instead of 2012-2018. Dash lines are 95% confidence interval of the estimates.

Figure C.14: Robustness Check With 2014-2018 Sample (Continued)