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The long and short of it: The impact of Apprehended Domestic Violence Order duration on offending and breaches

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AIM

To examine whether longer apprehended domestic violence orders (ADVO) are associated with changes in domestic violence (DV) offending and ADVO breaches.

METHOD

A dataset of 13,717 defendants who were placed on an ADVO after a DV incident between January 2016 and April 2018 was extracted from the NSW Bureau of Crime and Statistics and Research's ADVO database. This included 10,820 defendants subject to a final 12-month order, and 2,897 defendants subject to a final 24-month order. We utilised an entropy balancing matching approach to ensure groups of defendants subjected to differing ADVO lengths were comparable and implemented an event study analysis to examine quarterly differences in offending outcomes in the three years after the start of the order. In doing so, we were able to examine relative differences in offending in the first 12 months (where both groups were subject to an ADVO), the second 12 months (where only the 24-month ADVO group were subject to an ADVO), and the third 12 months (where neither group were subject to an ADVO).

RESULTS

In the 12 to 24 month period, where ADVOs with a longer duration were active and shorter ADVOs were not, longer ADVOs were associated with increased breach offending and decreased DV offending. Specifically, in the 5th, 6th and 7th quarters after the beginning of a final order we observed 3.4 percentage points (p.p.), 4.1 p.p., and 2.3 p.p. increases in defendants breaching their ADVO, respectively. When considering baseline rates of breaching, this represents relative increases of 79% to 161%. In the 5th, 6th, and 7th quarters after the beginning of a finalised order, longer ADVOs were associated with respective 1.8 p.p., 2.0 p.p., and 3.1 p.p. decreases in DV offending, reflecting relative decreases in DV offending by 41% to 59%. We find no group differences in DV offending or breaches in the subsequent 12 months, when ADVOs for both groups had expired. While the study examined multiple factors related to both longer ADVO length and offending, we cannot exclude the possibility that unobserved factors may be influencing our results.

CONCLUSION

Relative to 12-month ADVOs, 24-month ADVOs were associated with an increase in the probability that an offender breaches the conditions of their ADVO, and a decrease in the probability that an offender commits a proven DV offence.

KEYWORDS

Apprehended violence orders (AVO) | Domestic violence | Recidivism | Victims | Policing

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INTRODUCTION

Although Australia has experienced a long-term decrease in crime rates across most major crime categories, domestic violence (DV) remains a persistent concern (Weatherburn & Rahman, 2021). It has been estimated that more than one in four adult women and around one in eight adult men have experienced intimate partner or family violence since the age of 15 (Australian Bureau of Statistics (ABS), 2023). Successive waves of victim surveys indicate that neither rates of DV victimisation nor subsequent rates of reporting to police have changed dramatically over the past decade (Freeman, 2023).

The pervasiveness of DV has attracted considerable attention and resourcing from all levels of government. In 2015, the New South Wales (NSW) Premier announced a target to reduce the rate of DV reoffending by 25% in the following five years (NSW Government, 2015). A recommitment to the target in 2019 aimed to reduce DV reoffending by the same amount by 2023 (NSW Government, 2023). More recently, the National Plan to End Violence against Women and Children 2022-2032 ((The National Plan); Australian Department of Social Services (DSS), 2022) set out a broad suite of actions that the Federal and State governments intend to take to combat DV. These are classified under four main categories: 1) primary prevention activities, which aim to prevent DV before it occurs; 2) early intervention activities, which seek to intervene at early stages to reduce the likelihood of further escalation and harm; 3) responses to DV incidents, including criminal justice and other protective responses to individual incidents of DV; and 4) rehabilitation and recovery programs, which aim to reduce immediate and long-term DV offending and provide longer-term support to victims.[1]

Protection orders for victims of domestic violence

One of the most established and widely available tools for responding to DV incidents are victim protection orders (Taylor et al., 2017), known in NSW as Apprehended Domestic Violence Orders (ADVOs). ADVOs are civil orders issued by the NSW Local Court which aim to protect a person in need of protection (PINOP) from further violence. Each order contains a set of conditions which are designed to protect the PINOP and can stipulate if and how the defendant can contact the PINOP, among other restrictions.[2] A breach of these conditions is a criminal offence and can attract severe penalties, including custody (see Donnelly & Trimboli, 2018). ADVOs offer several advantages relative to prosecution as a response to DV incidents. First, they provide a legal mechanism for authorities to intervene relatively quickly to prevent additional violence. Second, the set of conditions contained in each order may allow for a more tailored approach to ensure victim safety compared to the options available in criminal sentencing. Third, they are arguably more accessible than a prosecution because the standard of proof required for an ADVO is significantly lower than that required for a conviction. As a result, orders are able to protect people experiencing a variety of DV behaviours, including those that may not meet the threshold required for conviction. Last, by that same token, ADVOs may also be more widely used by victims who are concerned about the personal costs of lengthy criminal justice processes.

While protection orders are widely considered to play an integral role in maintaining victim safety, they are not without criticism or cost. Critics of the scheme have argued that the protection order system mirrors the criminal law's narrow understanding of DV, which focuses on discrete incidents and visible forms of violence (Wangmann, 2012). This restricts the kinds of DV behaviours that justify the issuance of protection orders and limits the forms of violence which protection orders are designed to prevent. By failing to recognise the inconspicuous nature of DV offending, protection orders may not provide an adequate response to those experiencing unreported or ongoing violence. Others have expressed dissatisfaction with the inability of protection orders to impede the actions of perpetrators intent on committing violence (Auerbach, 2016). Although protection orders may allow for a heightened criminal justice response to DV offending before it escalates, their effectiveness is dependent on the willingness

1 Readers interested in existing approaches for the management of DV are directed to Dowling et al. (2018a) for a review of police-led responses to DV, and Payne (2006) for information regarding family and DV courts.
2 See Appendix A for the full list of standard ADVO conditions which can be applied in NSW.

of victims to report DV behaviours, and the adequacy of police responses to breaches. While Voce and Boxall (2018) find that victims who contact police are often the most susceptible to DV, Dowling et al. (2018a) report that protection orders are more likely to be enforced after more serious and recurring breaches. The authors also find that that the administrative burden of applying for protection orders often discourages police from making applications.

Beyond outcomes for victims, some have expressed concerns that protection orders may systematically disadvantage offenders. Specifically, questions have been raised regarding the criminalisation of otherwise non-criminal behaviours through ADVOs, which may lead to unintended "net-widening", where defendants accumulate a non-trivial criminal offending record due to technical breaching (see Douglas & Fitzgerald, 2018; Ringland & Fitzgerald, 2010).

The effectiveness of protection orders in improving victim safety

Although they are an integral part of the criminal justice response, there is limited rigorous evidence on the effectiveness of protection orders in improving DV-related outcomes. An inclusive systematic review by Cordier et al. (2019) examined reoffending rates after protection orders were issued across 19 U.S. studies and one Swedish study. This research found that, on average, between 28% and 34% of defendants committed a domestic violence offence whilst the protection order was in place. Despite this, studies that collected victim survey/interview data found that most victims perceived that their protection order was fairly, somewhat, or very/extremely effective. However, only four of the studies reviewed made use of a counterfactual group of domestic violence victims without an active protection order. Three of these studies concluded that victims with protection orders experienced similar outcomes to those without. The remaining study found that protection orders, when paired with a comprehensive community response program involving counselling, community corrections, and police interventions, increased the likelihood of reoffending. This result may be driven by increased detection of DV due to community supervision (see Doleac, 2018), and does not necessarily indicate that protection orders on their own increase the risk of further offending.

A more selective meta-analysis conducted by Dowling et al. (2018b) examined four U.S. and U.K. studies of the effectiveness of protection orders, all of which included control or matched comparison groups.[3] Taken together, the results showed that although around half of victims on protection orders experienced some form of re-victimisation, protection orders were generally associated with a small reduction in the frequency of DV behaviours. Additionally, the authors found preliminary evidence to suggest that protection orders may promote a de-escalation of violence to less severe and non-physical forms of abuse. However, these studies may be susceptible to selection bias because of how the control groups were defined. For example, McFarlane et al. (2004) compared those who received an order with those who applied for, but were not granted, an order, with the majority in the latter group withdrawing their application before their court date. If the reason for withdrawal is correlated with future re-victimisation (such as might be expected in the presence of coercive control behaviours), then this selective attrition may unduly bias the estimated effect of protection orders upwards.

Australian research investigating the influence of protection orders on offending and victim safety is limited. There are however two notable studies which focused on victims' experiences with the ADVO scheme in Australia. Firstly, Trimboli and Bonney (1997) interviewed 285 victims granted ADVOs in the NSW Local Court, and re-interviewed these victims one, three and six months after the order was served (constituting subsamples of 201, 115 and 59 victims, respectively). They discovered that throughout the six-month period after the protection order was served on the defendant, victims experienced sustained reductions in physical assault, stalking, and other forms of intimidation and harassment. However, as this study also did not include a control group of victims who were not on protection orders, it is not possible to determine whether this improvement in outcomes is a consequence of the protection afforded by the ADVO or some other contemporaneous factor. Further, the group of victims surveyed

3 Of the four studies reviewed by Dowling et al., (2018b), three were different to the studies that involved counterfactual groups in the meta-analysis by Cordier et al., (2019).

may not be representative of all victims protected by ADVOs. Secondly, Young et al. (2000) used data from a large national survey to explore the experiences of 493 women aged 18 to 23 years who reported being the victim of physical violence by a partner. Nearly two-thirds (338) of these young women had sought a legal protection order to stop the violence. Comparing the experiences of women with and without a protection order, Young et al. (2000) found that ADVOs were associated with a reduction in the severity, but not the frequency of reoffending. However, the study did not control for the fact women who experienced more severe levels of violence were more likely to seek legal protection as well as other relevant victim and offender characteristics. Additionally, the authors were not able to fully verify whether trends in victimisation were similar before protection orders were issued, as they did not restrict observation periods before and after protection to be the same duration. Given the methodological limitations of both these studies, it remains unclear to what extent protection orders applied in an Australian context work to improve victim safety.

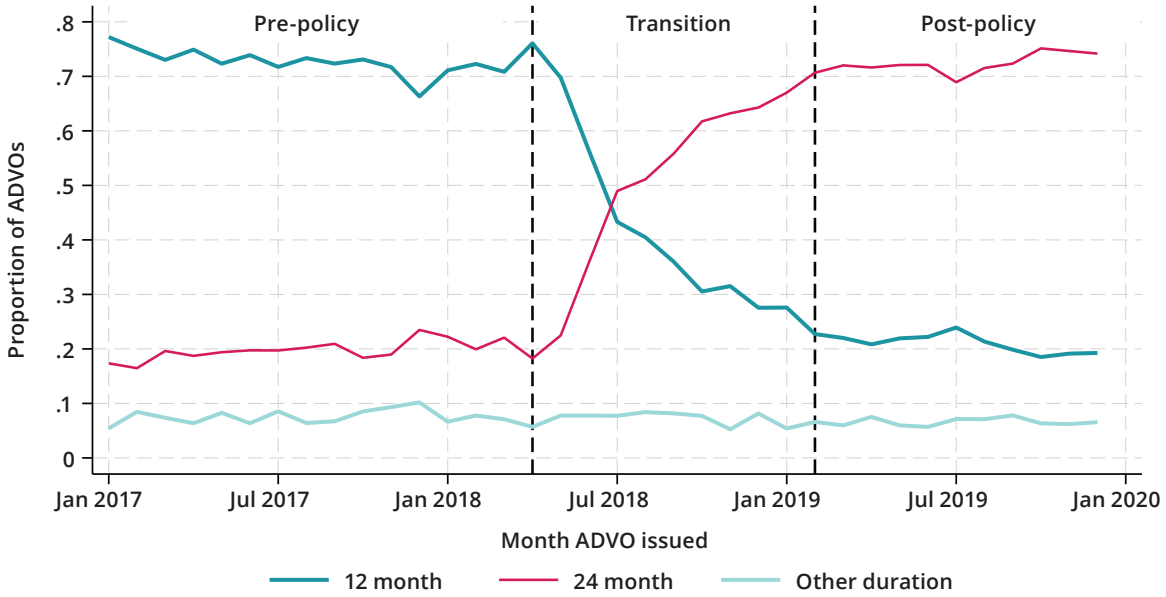
Unsurprisingly, literature on whether lengthening the duration of protection orders reduces DV is even more scant. The evidence in support of lengthening protection orders tends to be qualitative. Taylor et al. (2017) conducted semi-structured qualitative interviews with 20 DV victims and 20 representatives from agencies who provide victim support and legal services in Australia. The participants expressed the view that the onus should not be on the victim to produce evidence as to why a protection order should be renewed, and that the nature of DV should necessitate enduring protection of the victim. Building on these ideas, Taylor et al. (2017) further examined consistency in protection order duration between Australian states and found that although victims could register the same protection order in another state or territory (rather than applying for a new order), difficulties during the registration process often eroded victims' feelings of safety. Separately, Stoever (2014) compared the consistency of state-based victim protection order legislation across the United States, finding that existing practices regarding the length of protection orders were insufficient. For example, she argued that in some contexts, the extension of protection orders requires evidence of violence occurring during its duration. This could be problematic because the existence of an order may work to eliminate violence, which would, in turn, preclude an extension. As a result, Stoever (2014) argues that existing practices for extending protection orders may disadvantage those who they are intended to protect. Further, she reasoned that by reducing the frequency of protection order court proceedings, longer orders could improve victim safety by limiting contact between victims and abusive partners. This is consistent with other research indicating that victims on protection orders experience a greater frequency of subsequent DV if they maintain a relationship with their abuser (see Russell, 2012).

Although less directly relevant, the literature examining the timing and frequency of domestic violence offending also provides insight into the potential for longer ADVOs to reduce DV. Although qualitative researchers have argued that lengthening a protection order may improve victim safety, quantitative research into the timing of DV offending suggests that further violence occurs soon after a protection order is issued. For instance, Klein (1996) tracked 663 defendants on restraining orders in Massachusetts over a two-year period. He found that almost half of the victims were re-victimised within two years and that re-victimisation was most likely to occur thirty days after the protection order was issued. Similarly, Morgan et al. (2018) analysed reoffending behaviours of 1,099 Tasmanian offenders involved in a DV incident attended by police, finding 14% of the sample reoffended within 60 days, and 23% recorded a subsequent DV offence within 6 months. Further, those who reoffended within 60 days were more likely to be repeat reoffenders than those who reoffended after this period (41% vs 20%, respectively). Separately, Young et al. (2000) interviewed 121 young women who had been subject to physical violence and had sought legal protection (involving contacting police and/or obtaining a protection order). The vast majority (90%) of the interviewees reported that the violence had ceased within 12 months. This aligns with a systematic review conducted by Benitez et al. (2010), who analysed 26 US studies on protection order effectiveness, finding that the chances of a breach are highest in the three months after an order is granted. Taken together, these studies conclude that the frequency and risk of re-victimisation and breaches may be highest in the short period after a protection order is issued. This suggests that lengthening protection orders may have minimal impact on the incidence of DV.

The current study

To enhance the effectiveness of ADVOs in NSW, in April 2018 the NSW Police Force directed officers to request a standard 24-month duration in all police applications for final ADVOs. The change in police practice was implemented after recommendation by the NSW Domestic Violence Death Review team that the Attorney General consider mechanisms to increase the length of protection orders in domestic violence matters (NSW Government, 2017). This resulted in a gradual increase over time in the proportion of final ADVOs granted by the courts which had a duration of 24 months. Figure 1 shows this change in default ADVOs issued by police. Around 70% of ADVOs were 12 months long, while 20% were 24 months long. After the policy was introduced, the proportion of 24-month ADVOS gradually increased to around 70% while the proportion of 12-month ADVOS slowly decreased to around 20% by February 2019. Following this change in default ADVO length, a legislative amendment[4] was introduced in November 2018 to formalise the change in default ADVO length. The amendment required that in the absence of a specific court order the duration of a final ADVO should be set to 24 months (instead of the existing 12-month default period).

Figure 1. Proportion of 12 and 24-month ADVOs around change in police practice for issuing ADVOs



Reform date = 01 April 2018

To date no studies have considered whether the 2018 legislative amendments (which in effect doubled the mean length of ADVOs in NSW) resulted in reduced DV offending. The anticipatory action taken by police prevents a direct comparison of orders issued immediately before and after the commencement of the new legislation in terms of their impact on victim safety. For further explanation of the difficulty of directly evaluating the change in police practice, see Appendix B.

While we were not able to directly evaluate the reform, the current study aims to shed light on the impact of ADVO length on DV offending by comparing rates of offending and order breaches, in the two years before and three years after an order commenced, for defendants issued with 12- and 24-month ADVOs. In doing so, we look at the influence of longer ADVOs on DV offending and ADVO breaches in each quarter after the issuance of an ADVO. While it is true that those placed on longer ADVOs will have a greater opportunity to breach their protection orders, a dearth of research on this topic means the

4 Schedule 1 of the *Crimes Legislation Amendment Act 2018* (NSW) amended ADVO provisions in the *Crimes (Domestic and Personal Violence) Act 2007* (NSW). Other than changing ADVO length, the reform also 1) enabled the Local Court to set the duration of a final ADVO for adult defendants as an indefinite period; 2) provided legislative guidance on the duration of an ADVO that could be sought by an applicant and 3) enabled police to provisionally vary the conditions of an ADVO.

magnitude of this increase is unknown. Considering that most offending is known to happen within the first few months after an ADVO is issued, it is not apparent that breach rates will remain high after the first 12 months of an ADVO. Knowing how and when breaches occur may provide insight into the mechanisms by which ADVO duration influences DV.

METHOD

There are likely to be factors which are correlated with both the length of ADVO a defendant receives and risk of future DV offending. Without accounting for these factors, we might find that 24-month orders increase DV offending relative to 12-month orders, but this could be due to differences in defendant characteristics rather than order duration. We also know that victims are most at risk soon after an ADVO is issued. We therefore need a method that can control for differences in both observable and unobservable characteristics of defendants on longer and shorter ADVOs, while also capturing the dynamic impacts of ADVO duration on DV behaviours.

To ensure that those placed on a 24-month ADVO were comparable with those issued a 12-month ADVO, we utilised a matching technique. We then implemented an event study analysis using this matched sample to identify whether longer ADVO length was associated with changes in offending over time. Matching allowed us to compare outcomes for groups who were similar across various sociodemographic and criminogenic characteristics that were correlated with the risk of offending and/or the length of the ADVO issued. The use of an event study analysis allowed us to remove biases from any additional unobserved differences between those on 12- and 24-month ADVOs which matching cannot account for.[5]

Data

The data used in this study are derived from three sources:

1. **NSW Bureau of Crime Statistics and Research's (BOCSAR) ADVO data:** An extract from BOCSAR's ADVO database, providing information on the defendant, persons in need of protection, and active dates for all ADVOs created or amended in NSW from January 2016.
2. **NSW Police Force's Computerised Operational Policing System (COPS) data:** An extract of police incidents from the COPS database, detailing dates of reporting and occurrence, the type of incident, and the person of interest (POI) attached to the event. The extract also provides information regarding the sociodemographic characteristics of POIs, including their age, gender, Aboriginality, and the relative socioeconomic disadvantage and remoteness areas associated with their residential address at the time of the incident.
3. **BOCSAR's Re-Offending Database (ROD):** An extract drawn from ROD, which provides information on youth justice conferences, police cautions, and court appearances finalised in NSW between January 1994 and January 2023. The extract used in this study provides information regarding dates of offending, offence type and court outcomes.

We began with an initial extract of 59,223 final ADVOs with commencement dates between 01 January 2016 and 01 April 2018. Importantly, this time period was selected to avoid potential confounding from the ADVO duration reforms and COVID-19.[6] These data were linked with police and court data using a combination of probabilistic matching and direct matching on source system linkage keys. To identify our analysis sample, we applied several exclusion criteria. Firstly, we excluded 11,898 ADVOs where

5 Note that our approach estimates how longer ADVOs influence reoffending outcomes before the ADVO reform was introduced, rather than exploiting variation around the ADVO length reforms. Details on why alternative estimation strategies were not feasible are available in Appendix B.

6 We extracted final ADVOs with commencement dates from January 2016 as this corresponded with the beginning of the AVO database and was the earliest commencement date available. We then analysed final ADVOs with commencement dates before April 2018 to ensure that defendants were not issued ADVOs after the rollout of the ADVO duration reforms (see Appendix B) as this may have threatened the validity of our event study design. This is because unobservable differences between defendants on 12 and 24-month ADVOs may have changed after these reforms.

a defendant had multiple overlapping orders because in these cases we were unable to identify the specific order that was breached.[7] Secondly, as custom variations in order length may be associated with offending behaviour or circumstances that are unobservable in our study, we excluded a further 4,563 ADVOs that were not set to the default 12- or 24-month durations.[8] Thirdly, we selected the index ADVO as the first ADVO for each defendant within the sampling period that met the above criteria excluding a further 1,022 orders which were new orders for the same defendant. We were then left with an extract of 41,740 defendants, of which 33,276 defendants were subject to a 12-month ADVO and 8,464 were subject to a 24-month ADVO. Fourthly, we linked this sample with the COPS database to identify a group of ADVO defendants with at least one prior police recorded DV incident[9] for whom we could observe a larger number of sociodemographic characteristics.[10] Understanding the offending of this group is arguably more policy relevant as they are known to pose a higher immediate risk to their victims and are more likely to commit further offences. This restriction means that we do not focus on the influence of ADVO length for all ADVO defendants. As a robustness test, we examined offending outcomes for the wider group of ADVO defendants regardless of prior DV offending, although we could not observe sociodemographic characteristics for all of these defendants (see Appendix E2).

After applying these criteria, the final dataset contained information for 13,717 DV offenders placed on a final ADVO after a DV incident between January 2016 and April 2018. This included 10,820 defendants who were placed on a 12-month final ADVO and 2,897 who were placed on a 24-month ADVO. For these groups, we constructed a quarterly longitudinal panel dataset capturing DV offending and ADVO breach offending in the eight quarters before and 12 quarters after the commencement of the index ADVO.

Variables

Outcome variables

The study considered two main outcome variables:

1. **DV offending:** A variable equal to one if an offender committed at least one proven[11] DV-flagged offence (excluding ADVO breach offences as defined below).
2. **ADVO breaches:** A variable equal to one if an offender recorded at least one proven ADVO breach offence in the observation period. This includes DV-flagged offences with an Australian and New Zealand Offence Classification (ANZSOC[12]) group of 1531 relating to a breach of violence order, and offences with lawpart codes relating to ADVO breaches, including 1207, 1208, 62079, 65020, or 69120.

Note that ADVO breaches are also DV offences. We measure DV offending and ADVO breaches separately as to capture the seriousness of reoffending.

7 Because the dataset used in this study was limited to ADVOs created or amended from Jan 2016, there is likely a small number of overlapping orders that we were unable to observe (those beginning before this period). Consequently, we detect a low baseline rate of ADVO breach offending in periods where it appeared from our data that offenders were not subject to ADVOs. Since we cannot consistently identify these defendants, we are unable to exclude them from our analysis without biasing the results of the study.
8 As a proportion, around 7.7% of orders were excluded from our analysis as they were not set to default durations of 12 or 24-months and excluded from our analysis.
9 We identify an index DV incident as being associated with an ADVO if it occurred within one day of the ADVO start date.
10 The NSW Police only record a limited set of information for those who receive an ADVO without being involved in a police incident. These are: gender, age, Aboriginality and Police Area Command (PAC).
11 A proven offence in our study is recorded as any youth justice conference, police caution, or proven court appearance. We count offences using the offence date. This means an offence is counted in the quarter when the offence was committed, rather than the quarter when it was proven.
12 See Australian Bureau of Statistics (2011).

Explanatory variables

A variety of explanatory variables were considered for matching defendants placed on 12- and 24-month ADVOs.

1. **Sociodemographic characteristics**

    - Gender: Whether a defendant was male or female, or had gender recorded as unknown.
    - Aboriginality: Whether a defendant had ever been recorded as an Aboriginal and/or Torres Strait Islander person at any police incident.
    - Age at the start of the order: A defendant's age in years at the date they began their ADVO.
    - Socioeconomic disadvantage: The Australian Bureau of Statistics (ABS) Socio-Economic Indices for Areas (SEIFA) Index of Relative Disadvantage associated with the postcode of an offender's residence, segmented into quartiles (ABS, 2016a).
    - Remoteness of residence: The ABS remoteness area associated with the postcode of an offender's residence.[13]

2. **Time and location of the DV incident**

    - Month and year of order start: Indicator variables representing the month and year that an offender began their final ADVO.
    - Police Area Command (PAC) of index DV incident: Indicator variables representing the PAC responsible for the defendant's index DV incident.

3. **Prior criminal offending**

    - Number of prior offences: The number of proven offences in the five years prior to the ADVO start date.
    - Number of prior DV offences: The number of proven DV-flagged offences in the five years prior to the ADVO start date.
    - Number of prior prison sentences/control orders: The number of finalised court appearances at which a defendant was given a full-time prison sentence or juvenile control order in the five years prior to the ADVO start date.
    - Number of breaches of violence orders: The number of proven breach of a violence order (ADVO or APVO) (ANZSOC 1531) offences recorded in the five years prior to the ADVO start date.
    - Number of prior property offences: The number of proven property offences (ANZSOC 07, 08 or 09) in the five years prior to the ADVO start date.
    - Number of prior violent offences: The number of proven violent offences (ANZSOC 01, 02, 03 or 06) in the five years prior to the ADVO start date.
    - Number of prior drug offences: The number of proven drug offences (ANZSOC 10) recorded in the five years prior to the ADVO start date.

Note that we do not include variables relating to ADVO conditions, as such data was only available for 64% of defendants in our sample. As a robustness test, we matched on the ADVO condition data that we could observe (see Appendix E1).

13 Based on the Accessibility and Remoteness Index of Australia (ARIA; ABS, 2016b). Remoteness measures in our study are categorised as Major cities, Inner regional, Outer regional, Remote or Very remote, and missing.

Empirical approach

Constructing observably similar offender groups

A simple comparison of outcomes for defendants with different ADVO lengths is problematic if the groups differ on unobserved factors that also influence DV offending. For example, offenders who perpetrate coercive control against their partner may be more likely to receive a 24-month ADVO. Where no legal action was taken by police, these behaviours would not be observable in our dataset.[14] If defendants who engage in these behaviours are also more likely to commit further offences, then our estimate of the effect of longer ADVOs on DV offending would be biased upwards.

To address potential selection bias, we used entropy balancing (a form of matching) to identify a set of defendants placed on 12-month ADVOs who were comparable on observables with a group of defendants who had been placed on 24-month ADVOs. The simplest form of matching would be to pair each defendant on a 24-month ADVO with the defendant on a 12-month ADVO who was the most similar in terms of demographic characteristics and prior offending history. However, the idea of "similarity" is not well defined when matching across multiple observable characteristics. To overcome this concern, we utilise an entropy balancing approach (Hainmueller, 2012) to calibrate a doubly robust[15] set of matching weights that identifies reweighted groups that match as closely as possible on the explanatory variables listed previously and trends in pre-treatment outcomes. We match on pre-treatment outcomes as there is evidence that doing so reduces bias when pre-intervention trends in group mean outcomes are not parallel (see Cefalu et al., 2020).[16]

Entropy balancing provides several notable improvements over other commonly used matching techniques, such as nearest neighbour matching. Firstly, it eliminates the need for researchers to manually iterate over matching models until covariates are adequately balanced, lessening modelling bias. Secondly, an entropy balancing approach delivers a balance of observable characteristics equal or better than that achieved using propensity score matching (Hainmueller, 2012). However, this approach may not be appropriate if there are only a small number of defendants on 12-month ADVOs who have similar characteristics to defendants on 24-month ADVOs. In this case, results may be skewed by the outcomes of a relatively small number of defendants, who would receive large matching weights. To test whether this is an issue, we conducted a diagnostic check to ensure that defendants on 12- and 24-month ADVOs were comparable. This involved testing whether the standardised bias[17] (which enumerates differences in observable characteristics between groups) was reasonably small after matching, observing if matched pre-trend outcomes were similar for defendants on 12- and 24-month ADVOs, and examining whether any 12-month ADVO offenders received unreasonably large matching weights.[18]

Despite the strength of our matching approach, it is plausible that there are unobservable differences between groups that we are unable to directly account for in the data. As discussed above higher risk offenders (e.g., those who would be more likely to commit a further offence) were prioritised for placement on 24-month rather than 12-month ADVOs. Consequently, any remaining bias from unobserved omitted variables after matching in our analysis would lead to an overestimate of the likelihood that those on 24-month orders would reoffend.[19]

14 Although legislation passed to criminalise coercive control in November 2022 and is expected to come into force before July 2024, our dataset only includes data on offending until 01 April 2021. As a result, we were not able to observe such behaviours in our study.
15 When used in isolation, propensity score models using both linear and logistic specifications are only unbiased if the structure of the statistical models used to estimate the propensity score are correct. Zhao and Percival (2017) demonstrate that entropy balancing simultaneously fits a logistic model for the propensity score and a linear regression model for the outcome. Only one of these two models need to be correctly specified to obtain an unbiased estimator, a concept known as double robustness.
16 As a robustness test, we matched only on observable characteristics of offenders and not on pre-intervention trends. See Appendix E.3 for details.
17 Rosenbaum and Rubin's (1985) standardised bias is the most commonly used statistic to examine the balance of covariates between treatment and control groups. In our study, it is calculated as the difference in means between the 24-month ADVO and the matched 12-month ADVO group, divided by the standard deviation of the 24-month ADVO group. We adopt a conservative 10% threshold to determine whether covariates are balanced.
18 Parish et al. (2017) and McMullin and Schonberger (2022) argue that a reasonable limit for the size of matching weights may be between 20-30.
19 The directionality the bias can be inferred given what is known about the remaining unobserved confounders and the ADVO placement process (see Heckman, 1977). This idea follows the framework of Angrist and Krueger (1992), who argue that institutional knowledge about the process determining treatment can be used to make nuanced comparisons between groups. More recently, omitted variable bias frameworks have been implemented by Abdulkadiroglu et al. (2014).

Event study analysis

Since we were only able to match on variables that we could observe, it is possible that our matched groups were not comparable across all variables that influence our offending outcomes. For instance, we could not observe variables such as any unreported prior DV incidents or the nature of the relationship between the victim and offender. To reduce the influence of any unobserved confounders on our estimates, we employed an event study approach.[20] This involved comparing changes in outcomes over time for defendants issued 24-month ADVOs with changes in outcomes over time for the matched comparison group of defendants who received 12-month ADVOs. This removes biases from time confounders, as well as any remaining biases from unobserved differences between our matched groups.

For the event study analysis, we first transformed our data into a quarterly longitudinal panel of offending consisting of 12 quarters after a defendant's ADVO start date. We define our treatment effect as the impact of the extra 12 months that defendants on 24-month ADVOs experience relative to their 12-month ADVO counterparts. Specifically, we compared reoffending outcomes between offenders on 12- and 24-month ADVOs by estimating the following model:[21]

$$y_{it} = \alpha + \sum_{j=2}^{4}(\beta_j(Lag\ j)_{it} + \theta_j(Lag\ j)_{it} \times D_i) + \sum_{k=1}^{8}(\gamma_k(Lead\ k)_{it} + \tau_k(Lead\ k)_{it} \times D_i) + \tau D_i + X'_{it}\delta + \rho_i + \varepsilon_{it} \quad (1)$$

where y_{it} is a dummy variable which takes a value of 1 if an ADVO defendant i offends at time t and 0 otherwise. We include dummy variables for each lag j in quarters before the end of their first year on an ADVO (1 to 4 quarters after ADVO start) and leads k for the remaining quarters (5 to 12 quarters after ADVO start). The first lag $(j = 1)$ is omitted to capture the baseline difference between individuals who receive 12 and 24-month ADVOs just before the end of their first year on an ADVO. The indicator variable D_i captures the treatment group and is equal to one for those placed on a 24-month ADVO, and zero for those on a 12-month ADVO. X'_{it} is a vector of covariates relating to prior offences and demographics (as listed earlier in the Variables section), and p_i are PAC fixed effects. Our parameters of interest are the coefficients on the lead interaction terms, $\tau_{k'}$ which capture the effect of being on a 24-month ADVO relative to a 12-month ADVO at a particular lead quarter. For instance, τ_1 is the average change in reoffending for offenders on 24-month ADVOs, relative to those on 12-month ADVOs, in the first quarter of their second year on an ADVO. Similarly, τ_2 is the average change in reoffending for offenders on 24-month ADVOs relative to those on 12-month ADVOs in the second quarter of their second year on an ADVO, and so on.

This approach allows for a comparison of quarterly offending rates for defendants with different ADVO lengths across:

1. the first 12 months where both groups of defendants are on an ADVO;
2. the second 12 months where defendants on 12-month ADVOs are no longer on protection orders, while defendants on 24-month ADVOs have an extra 12 months left of their ADVO, and;
3. the third 12 months where neither group are on an ADVO.

Analysing offending rates within these periods allows us to test: 1) the similarity of offending and breach behaviours when both groups are under ADVOs; 2) the differences in outcomes among those who are on the additional 12 months; and 3) whether there is any evidence for a lasting deterrent effect of longer ADVOs.

20 See Clarke and Tapia-Schythe (2021) for more details.

21 Standard errors were clustered at the offender level to account for the fact that observations on the same offender are correlated, but independent between different offenders. We test the robustness of our findings by including additional offending variables and using alternative datasets and matching methods. Details are available in Appendices D and E.

Our event study approach only estimates a causal effect of the impact on ADVO length on reoffending if two conditions are met. Firstly, offenders on 12- and 24-month ADVOs must have similar patterns of offending during the first 12 months of an ADVO.[22] To determine whether this holds, we test the joint significance of the lag interaction coefficients (i.e., we test the null hypothesis that $\theta_2 = \theta_3 = \theta_4 = 0$, from Equation 1). Secondly, there must be no time-varying differences between defendants on 12- and 24-month ADVOs, such as changes in policing, that affect defendants on 12- and 24-month ADVOs differently. Even though we cannot test this assumption, we minimise any potential effects of time-varying differences by restricting our sample to the period before the ADVO length reforms commenced. We also match directly on the month and year of ADVO start date, to ensure that both treatment and control groups are equally exposed to unobservable differences in the policy and offending environment.[23]

RESULTS

Descriptive statistics

Characteristics of defendants on 12- and 24-month ADVOs

Table 1 compares the characteristics of defendants on 12- and 24-month ADVOs. Relative to defendants on 12-month ADVOs, those on 24-month ADVOs were more likely to be from socioeconomically disadvantaged areas (5.8 percentage points (p.p.) more likely to be in the most disadvantaged SEIFA quartile, 6.8 p.p. less likely to be in the most advantaged SEIFA quartile) and 8.0 p.p. more likely to be male. Also, around 31.2 p.p. of those on 12-month ADVOs and 35.7 p.p. of people on 24-month ADVOs were identified as Aboriginal, reflecting a 4.5 p.p. difference. There were very small differences in the age and remoteness of residence distributions between those on 12- and 24-month ADVOs. However, we do find that defendants on 24-month ADVOs had more extensive prior offending histories. In particular, defendants on 24-month ADVOs were 8.2 p.p., 5.9 p.p., 6.1 p.p., 2.4 p.p., and 1.8 p.p. more likely to have more than one prior proven offence, DV-flagged offence, violent offence, property damage offence and drug offence within five years, respectively, compared to those on 12-month ADVOs. Relative to defendants on 12-month ADVOs, defendants on 24-month ADVOs were also 9.3 p.p. more likely to have at least one prior prison sentence and 5.9 p.p. more likely to have breached an ADVO at least once within the last five years.

22 For simplicity, we do not test for parallel trends during the 2 years before an ADVO is issued, as we explicitly match on outcomes during this period, so they will be comparable by construction (See Figures 5 and 7).
23 See Appendix Figure C2 for an overview of time related matching.

Table 1. Sociodemographic and criminal justice characteristics of defendants issued ADVOs, by final order length

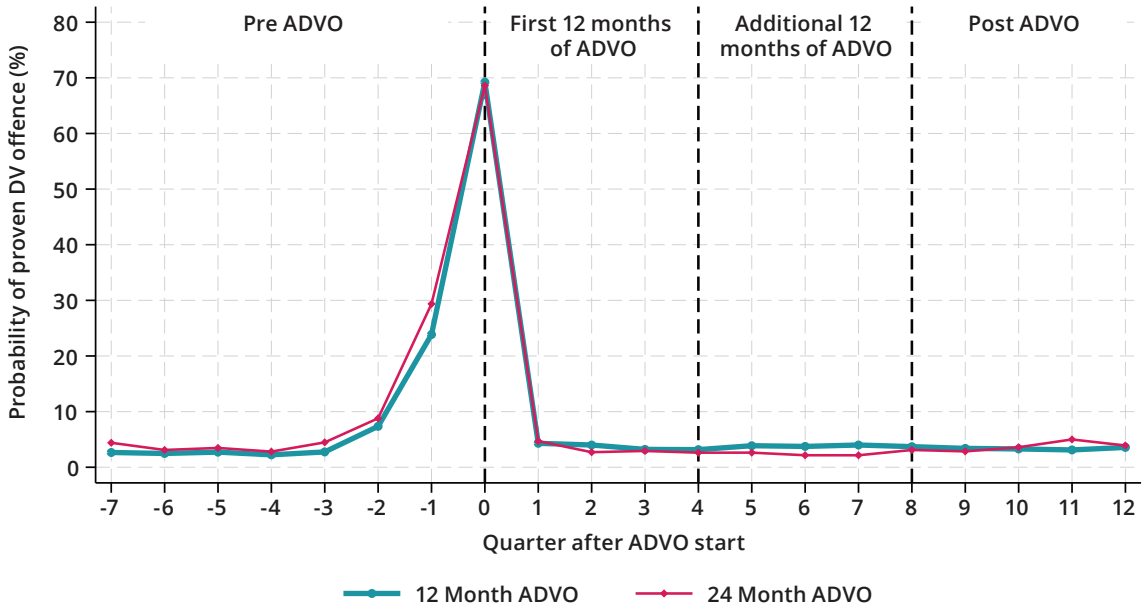
		12 Month ADVO (n=10,820)	24 Month ADVO (n=2,897)	Difference
SEIFA quartile of residence at index event	Quartile1 (most disadvantaged)	30.02	35.83	5.81***
	Quartile 2	28.13	28.48	0.34***
	Quartile 3	24.13	23.37	-0.76***
	Quartile 4 (least disadvantaged)	14.14	7.39	-6.75***
	Missing	3.58	4.94	1.36***
Age at index event	10-17	1.91	0.52	-1.40***
	18-29	37.60	35.00	-2.60*
	30-39	28.39	30.20	1.81*
	40-54	25.70	28.82	3.12***
	55+	6.35	5.42	-0.93***
	Missing	0.05	0.03	-0.01***
Aboriginality	Non-Aboriginal	68.72	64.27	-4.45***
	Aboriginal	31.18	35.69	4.51***
	Unknown	0.09	0.03	-0.06***
Remoteness of residence at index event	Major Cities	64.15	63.13	-1.02***
	Inner Regional	22.38	24.51	2.13*
	Outer Regional	8.23	6.56	-1.68**
	Remote	1.33	0.83	-0.50*
	Very Remote	0.34	0.10	-0.24*
	Missing	3.57	4.87	1.30**
Gender	Female	20.44	12.29	-8.16***
	Male	79.44	87.44	8.00***
	Unknown	0.12	0.28	0.16***
Proven offences in prior 5 years	0	28.21	29.17	0.96***
	1	38.64	29.48	-9.16***
	2+	33.15	41.35	8.20***
Sentenced prison episodes in prior 5 years	0	92.02	82.78	-9.25***
	1	4.34	8.84	4.49***
	2+	3.63	8.39	4.76***
Proven ADVO breach offences in prior 5 years	0	88.68	82.81	-5.87***
	1	8.54	12.36	3.82***
	2+	2.78	4.83	2.05***
Proven DV offences in prior 5 years	0	37.63	39.11	1.48***
	1	52.23	44.87	-7.35***
	2+	10.14	16.02	5.88***
Proven violent offences in prior 5 years	0	42.90	42.08	-0.82***
	1	46.37	41.08	-5.29***
	2+	10.73	16.85	6.11***
Proven property offending in prior 5 years	0	89.24	84.81	-4.43***
	1	6.53	8.53	1.99***
	2+	4.22	6.66	2.44***
Proven drug offending in prior 5 years	0	87.13	84.36	-2.76***
	1	8.98	9.94	0.96***
	2+	3.89	5.70	1.80***

Note. Values are reported in proportions. Stars indicate statistical significance at a variety of conventional thresholds of statistical significance: *p <0.05 **p <0.01 ***p <0.001.

DV offending and breach rates among all defendants issued 12- and 24-month ADVOs

We now examine the trends in DV offending for the unmatched groups of ADVO defendants. Figure 2 compares trends in the probability of a proven DV offence (excluding breaches), before and after the final ADVO issue date, for the defendants on 12-month and 24-month ADVOs. The "pre-ADVO" period comprises the eight quarters before a final ADVO started. Note that this could include the time where defendants were on provisional and/or interim ADVOs before a final ADVO was issued by the court. Following this, the "first 12 months of an ADVO" comprises the initial four quarters after the final ADVO commenced, where both groups of defendants were subject to the conditions of a final ADVO. The "additional 12 months of ADVO" period contains the subsequent four quarters where defendants on 12-month ADVOs were no longer on protection orders but defendants on 24-month ADVOs had an additional 12 months of their order remaining. The "post ADVO" period comprises the four quarters where no defendants are on ADVOs. As seen from Figure 2, the probability of DV offending before and after an ADVO was issued is remarkably similar for defendants issued 12- and 24-month ADVOs. During the pre-ADVO period, the probability of DV offending increased over time from around 5% to 65% for both groups, which was likely the trigger for an ADVO being issued.[24] In the two quarters after an ADVO was issued the probability of DV offending sharply declined to around 5% and remained relatively constant in the subsequent quarters.[25]

Figure 2. Quarterly rates of DV offending by people placed on 12- and 24-month ADVOs, unmatched sample



Next, in Figure 3, we plot trends in ADVO breach rates for unmatched defendants on 12- and 24-month final ADVOs.[26] In the pre-ADVO period, breach rates for both groups were relatively constant at around 2.5% until two quarters before the index ADVO commenced. Breaches were likely non-zero in this period as we were unable to exclude a small number of people who began an ADVO before January 2016 and were subject to the same ADVO in the study period. During the remaining pre-ADVO quarters and until the beginning of the ADVO (q-2 to q0), breach rates increased to around 8% and 12% for those in 12- and 24-month ADVOs respectively. This was likely driven by an increase in breaches of interim and provisional ADVOs before defendants were placed on final ADVOs. In the quarter immediately after

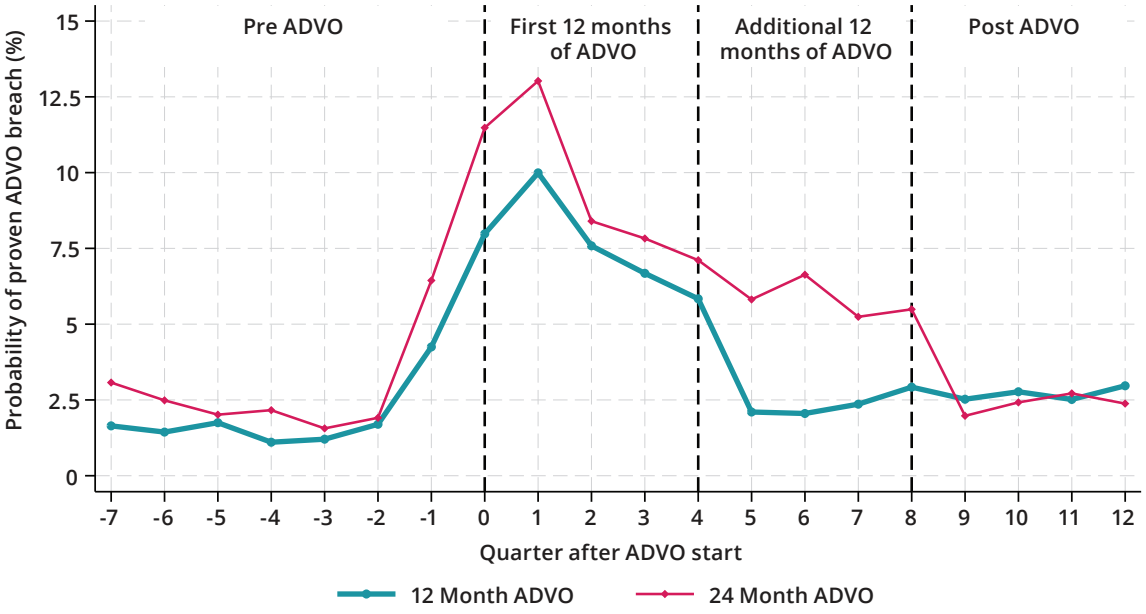
24 Leung and Trimboli (2022) investigate models which predict repeat intimate partner violence, finding that prior family and domestic violence is the most important predictor of revictimization.

25 While the average probability of any offending decreases after an ADVO is issued for those on 12 and 24-month ADVOs we do not include a comparison group of DV offenders who were not on ADVOs. Consequently, these trends do not provide any evidence regarding the effectiveness of ADVOs in general in reducing reoffending.

26 Note that the scale of Figure 3 illustrates breach rates between 0 and 15% and the scale of Figure 2 indicates offending rates between 0 and 80%. Thus, although relative differences between outcomes of defendants on 12 and 24-month ADVOs during the pre-ADVO period seem larger in Figure 3 than Figure 2, absolute differences are similarly small.

the ADVO commenced, around 10% of those on 12-month ADVOs breached their protection orders compared with around 14% of those on 24-month ADVOs. During the next three quarters (q2, q3 & q4), both groups exhibited similar trends in breach rates (at around 7%), but breaches were slightly higher for those on 24-month ADVOs. Not surprisingly, relatively large differences existed in the probability of an ADVO breach during the "additional 12 months" period (i.e., 4-8 quarters after the index ADVO commences), where defendants on 12-month ADVOs were no longer subject to a protection order but those on 24-month ADVOs were. More specifically, breach rates remained relatively constant at around 6% for defendants on 24-month ADVOs during this time but decreased to around 3% for defendants on 12-month ADVOs. Finally, around 3% of defendants in both groups had a proven breach offence during the post-ADVO period.

Figure 3. Quarterly rates of ADVO breaches by defendants placed on 12- and 24-month ADVOs, unmatched sample



Matched event study

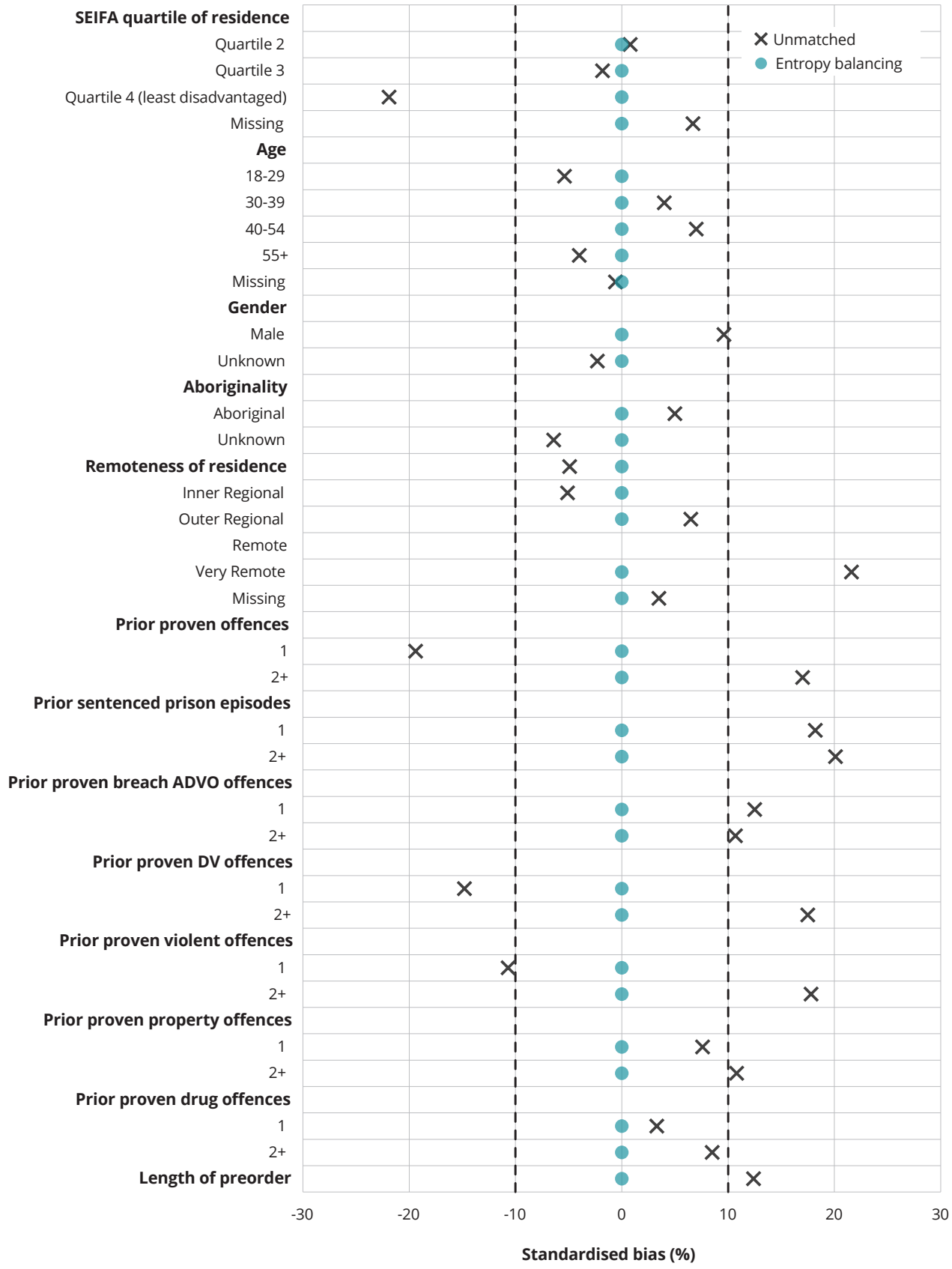
Diagnostic testing

Figure 4 presents the standardised bias before and after matching using the full set of covariates listed in the Variables section. Here the standardised bias is a measure of the difference between defendants on 12- and 24-month ADVOs on each covariate and is used to indicate the extent to which balance between the two groups has been achieved. The two dashed lines represent the 10% standardised bias threshold, which is often used as a cut-off to indicate "adequate balance" for a covariate.[27] Figure 4 shows that prior to matching, variables relating to age, remoteness of the defendant's residence and Aboriginality were balanced across the 12- and 24-month ADVO groups. However, variables relating to SEIFA quartile, gender, prior offending, and ADVO conditions were above the 10% threshold prior to matching which indicates that these variables were unbalanced across the groups. After entropy balancing was applied, all covariates were within the 10% standardised bias threshold, indicating that the matched 12- and 24-month ADVO groups were comparable across all observed characteristics.

A second issue that needs to be considered in entropy balancing is the composition of the weighted comparison group (i.e., how each individual defendant on a 12-month ADVO contributes to the weighted comparison group). To test this, we examined the distribution of entropy balancing weights. As seen in Appendix C, our maximum entropy balancing weight was less than six, indicating that our matching analysis was not unduly influenced by a few defendants on 12-month ADVOs with large matching weights.

27 See Austin (2009) for a discussion of covariate balance diagnostics for propensity score matching methods.

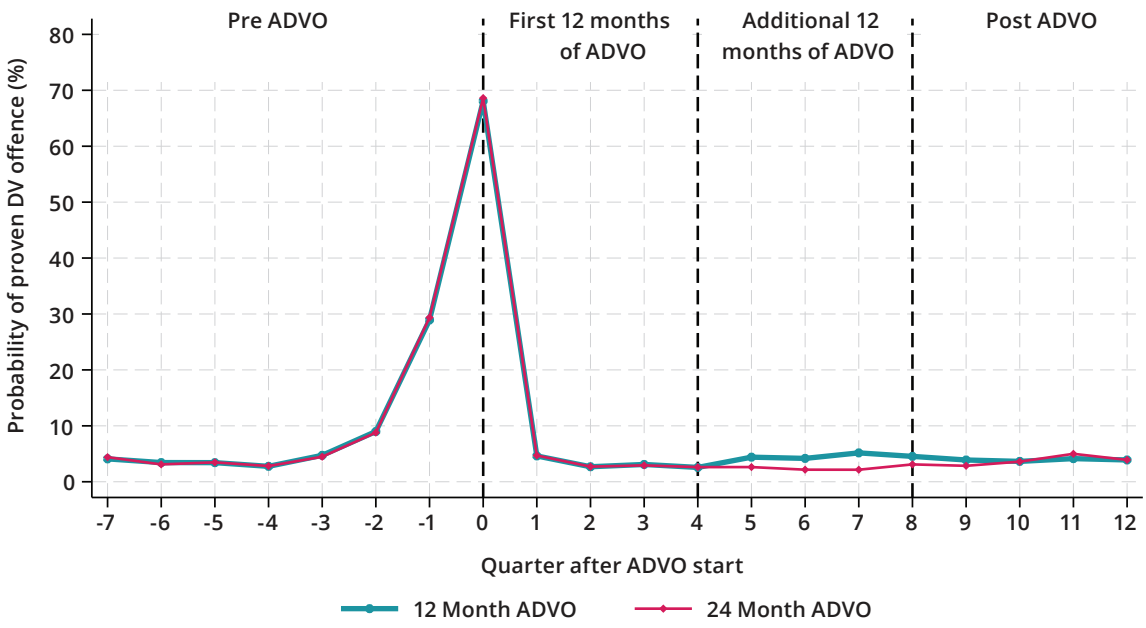
Figure 4. Standardised bias before and after entropy balancing



Differences in DV offending for matched defendants issued 12- and 24-month ADVOs

We now investigate the degree to which trends in outcomes were different between matched groups of defendants. Figure 5 illustrates trends in DV offending (excluding breaches) for 24-month ADVO defendants and a matched comparison group of defendants on 12-month ADVOs. Quarterly rates of DV offending were nearly identical for defendants on 12- and 24-month ADVOs from the pre-ADVO period until the end of the first 12 months of an ADVO. Specifically, for both groups of offenders, rates of DV offending increased from around 5% to 65% before an ADVO starts, then decreased to around 5% by the end of the first 12 months of an ADVO. Offending rates were so similar between groups in this period by design, as we match not only on covariates, but also on pre-intervention outcomes (where we define the intervention as the end of the first 12 months of the ADVO). From the fourth to the eighth quarter after an ADVO is issued (during the additional 12 month of the ADVO), rates of DV were higher for the matched group of defendants on 12-month ADVOs relative to defendants on 24-month ADVOs. Although the absolute difference in offending between these groups seems small during this time, relative differences in rates of DV were quite large. For instance, seven quarters after an ADVO was issued, offending rates for defendants on 12- and 24-month ADVOs were around 5.2% and 2.2% respectively. In absolute terms, this reflects a 3.0 p.p. difference, but represents a 58% decrease in relative terms. Finally, in the post ADVO period, the probability of DV offending returned to around 5% for both groups.

Figure 5. Quarterly rates of DV offending by people placed on 12- and 24-month ADVOs

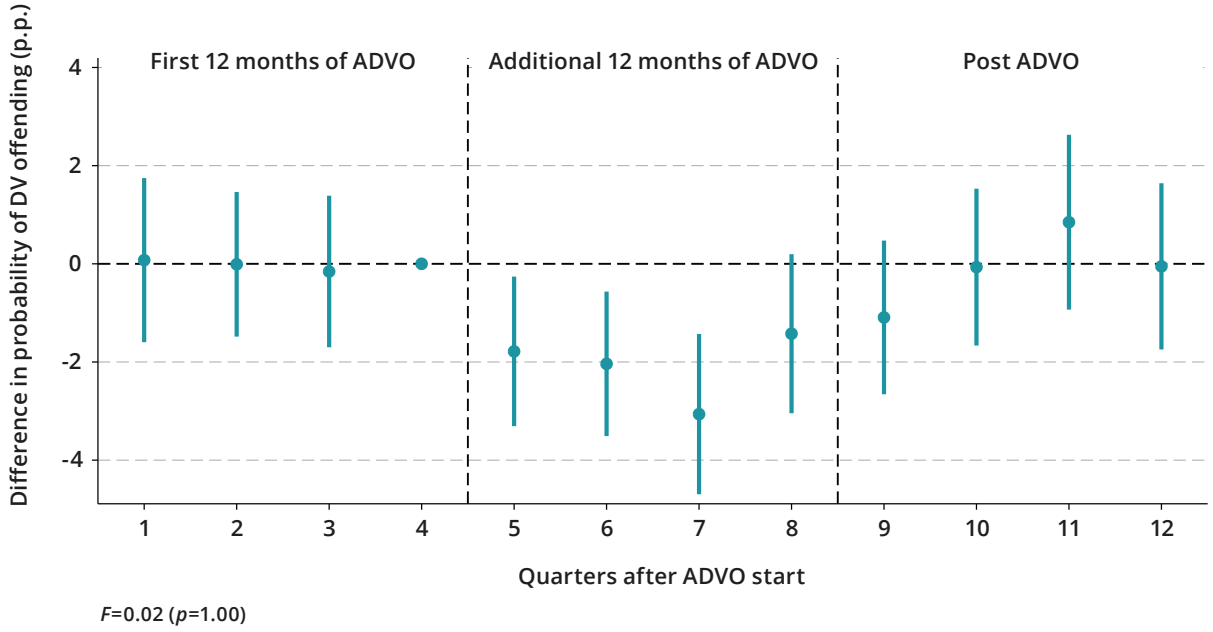


We determined whether differences in DV offending (excluding breaches) between matched defendants on 12- and 24-month ADVOs were statistically significant after controlling for relevant sociodemographic and criminogenic characteristics in our event study analysis. Figure 6 presents the event study estimates and 95% confidence intervals for the difference in the probability of DV offending in each quarter for the matched groups of ADVO defendants.[28] As an example the coefficient at quarter 5 is equal to -1.8 p.p. This means that in the fifth quarter after the beginning of an ADVO, those placed on 24-month orders were 1.8 p.p. less likely to commit a DV offence than similar offenders placed on 12-month orders. As only those placed on 24-month orders were still subjected to an ADVO in this period, this reduction in DV offending is associated with the effects of the longer ADVO. During the first four quarters after an ADVO starts, there were no significant differences in DV offending between those on 12-month and 24-month ADVOs. During the period where the 12-month ADVO defendants were not subject to a protection order,

28 The reported *F* statistic (and associated *p*-values) below the event study graph (and subsequent event study plots) test the significance of pre-period differences between the groups. We define the pre-treatment period as the first four quarters where offenders on 12 and 24-month ADVOs were both on protection orders. Quarter four is omitted as the reference quarter.

but the 24-month ADVO defendants were (i.e., 5-8 quarters after the ADVO starts), we find evidence that longer ADVOs were associated with a decrease in DV offending. Specifically, relative to 12-month ADVOs, 24-month ADVOs were associated with decreases in the probability of a proven DV offence by 1.8 p.p., 2.0 p.p., and 3.1 p.p., in the 5th, 6th and 7th quarters after an ADVO begins, respectively. When taking baseline rates of DV into account, this represents relative decreases in DV offending by 41%, 49% and 59% during these respective quarters.[29] Finally, in the post-ADVO period (9-12 quarters after an ADVO starts) there were no statistically significant differences between defendants on 12- and 24-month ADVOs.

Figure 6. Event study comparing DV offending outcomes in first 12, 24, and 36 months after ADVO start



F=0.02 (*p*=1.00)

Differences in ADVO breaches for matched defendants issued 12- and 24-month ADVOs

Next, we investigate whether longer ADVOs were associated with additional ADVO breaches. Figure 7 illustrates trends in ADVO breach rates for matched defendants on 12-month and 24-month ADVOs. Since we match on pre-treatment outcomes, quarterly rates of ADVO breaches are very similar for defendants on 12- and 24-month ADVOs during the pre-ADVO period (which captures breaches of provisional/interim/unobserved ADVOs), until the end of the first 12 months of an ADVO. Specifically, for both groups of defendants, breach rates begin at around 3% in the pre-ADVO period, increase to roughly 12% as the ADVO begins, then fall to approximately 7% at the end of the first year of the ADVO. During the additional 12 months of an ADVO (5th to 8th quarters), breach rates remain at around 5% to 7% for defendants on 24-month ADVOs but drop to roughly 3% to 4% for matched defendants on 12-month ADVOs. In the post-ADVO period, around 3% of defendants on 12-month ADVOs have a proven breach offence, while roughly 2.5% of those on 24-month ADVOs breach their ADVOs.

29 See Appendix F for detailed regression results including the calculation of marginal differences in offending.

Figure 7. Quarterly rates of ADVO breaches by defendants placed on 12- and 24-month ADVOs

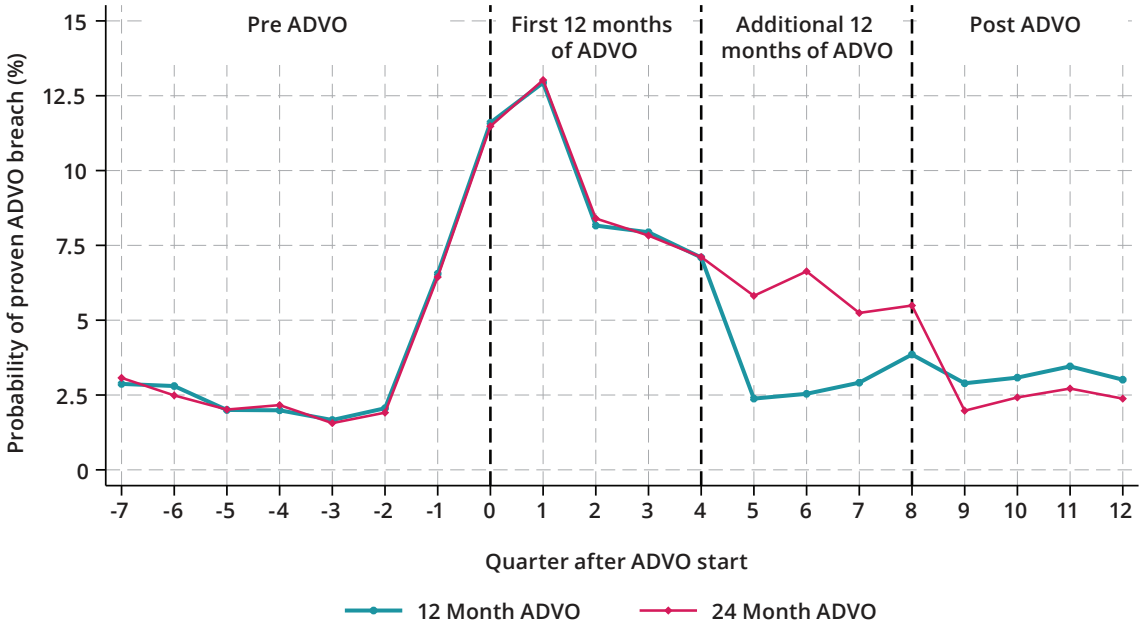
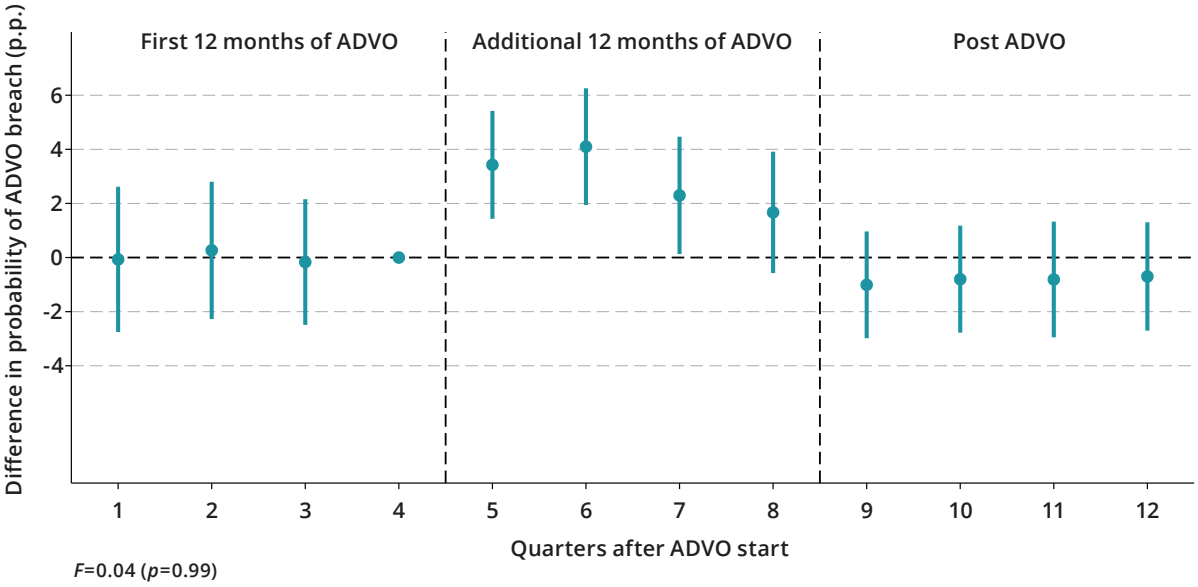


Figure 8 shows point estimates and associated 95% confidence intervals generated from our event study analysis, which tests whether differences between the two matched groups in each quarter were statistically significant after controlling for relevant offender and offence characteristics. During the first 12 months of an ADVO (i.e., 1-4 quarters after an ADVO starts), there were no statistically significant differences in breach rates for 12- and 24-month ADVO defendants.[30] However, 24-month ADVOs were associated with higher rates of ADVO breaches during the "additional 12 months" period where 24-month ADVO defendants were still subject to protection orders but those on 12-month ADVOs were not. In particular, 12-18 months (5th, 6th, and 7th quarters) after an ADVO starts, 24-month ADVOs were associated with 3.4 p.p., 4.1 p.p. and 2.3 p.p. increases in the probability of a breach, respectively. This represents a relative increase from baseline breach rates of 144%, 161% and 79%, respectively. During the remaining quarters (8-12 quarters after the index ADVO is issued), there was no statistically significant difference in breach behaviour for defendants on 12- and 24-month ADVOs.

Figure 8. Event study comparing ADVO breaches in first 12, 24, and 36 months after ADVO start



30 This is measured by the *F* test which rejects the null hypothesis that 24-month ADVOs are associated with a change in breach behaviour, relative to 12-month ADVOs during the first 12 months of an ADVO (p = 0.12).

DISCUSSION

The findings of this study contribute to the ongoing debate about the effectiveness of protection orders by examining whether setting the duration of an ADVO at 24 rather than 12 months enhances victim safety. To do this, we compared trends in DV offending for defendants issued a 24-month ADVO with observably similar defendants issued a 12-month ADVO using an event study approach. Our results suggest that extending the length of ADVOs from 12 to 24 months is associated with a reduction in the probability of further DV offending. Specifically, longer ADVOs were found to be associated with 1.8 p.p., 2.0 p.p., and 3.1 p.p. decreases in the probability of a proven DV offence (excluding breaches) in the 5th, 6th, and 7th quarters after the index ADVO commenced, respectively. This reflects relative decreases in DV offending in these periods of 41% to 59%. However, our analysis also demonstrated that longer ADVOs were associated with significantly higher breach rates. Our estimates indicate that defendants in the 24-month ADVO group were 3.4 p.p., 4.1 p.p., and 2.3 p.p. more likely to breach their ADVO during the 5th, 6th and 7th quarters after the beginning of a final order, respectively. When considering baseline rates of breaching, this represents increases of 79% to 161% during this time.

Although there are no equivalent studies which investigate the impact of ADVO duration on DV offending, our findings align with prior literature on the timing of DV offending and effectiveness of protection orders. For instance, past studies report that most DV reoffending occurs within the first three months after a protection order is issued (Benitez et al., 2010; Klein, 1996; Meloy et al., 1997; Poynton et al., 2016). In our study, we observe similar patterns in offending for those on both 12- and 24-month ADVOs, where rates of DV offending were highest in the quarter after an order was issued and much lower in subsequent quarters.[31] In light of this, our result that longer ADVOs were associated with reductions in DV during the same period where breaches increased suggests that extending an ADVO provides additional protection well after this high-risk period has ended. One explanation is that extended police surveillance and legal consequences associated with breach offending facilitates the de-escalation of DV behaviours which may have persisted in the absence of an ADVO. This is consistent with: 1) evidence that protection orders are linked with decreases in the frequency and severity of DV offending (Dowling et al., 2018b); and 2) our finding that longer ADVOs are associated with reductions in DV only during the period where those on shorter ADVOs are no longer protected. While it is argued that the recurring nature of DV necessitates protection orders which are even longer than 24 months (Stoever, 2014), it is unclear whether this would deliver similar benefits to what we report here. For example, if any deterrent effect of longer ADVOs diminishes over time, then the benefits of extending ADVOs would exhibit diminishing returns. This would need to be tested with a larger sample and longer follow-up period than we observe in our study.

It is unsurprising that those on longer ADVOs breach more often, as these defendants have more opportunity to commit breach offences. Notably, the absolute increase in ADVO breaches was around twice as large as the reduction in DV offending, and relative increases were up to three times as large. To the extent that breaches represent the criminalisation of offender behaviours that are unwanted or may harm DV victims, it is plausible that a higher rate of breaches may represent improvements to victim safety. For example, in our study period it is possible that ADVO breaches might reflect coercive controlling behaviours which may not have met the threshold for a DV offence. If so, longer orders may be important to more thoroughly and permanently disrupt patterns of coercive control. However, we cannot conclusively state whether detection and action in response to all of these additional breaches contributed to victim safety.

Although we cannot observe which ADVO conditions were breached in our study, previous work has found that the most frequently breached conditions included proximity to the victim's house/work address and telephone contact (see Trimboli & Bonney, 1997). Consequently, some of the extra breaches we observe could be the result of technical "net widening" behaviours or increased surveillance from

31 Note that although we find that DV offending rates decrease for offenders on both 12 and 24-month ADVOs after an ADVO is issued, this does not imply that ADVOs (regardless of duration) reduce DV offending. We are unable to assess whether placement on an ADVO decreases DV offending as we do not include a comparison group of DV offenders who are not issued ADVOs.

differential policing (see Douglas & Fitzgerald, 2018; Ringland & Fitzgerald, 2010). This is of concern as prior research has found that accumulating a criminal offending record is associated with a greater likelihood of imprisonment (Napier et al., 2015). Miller (2021) and Wheelock et al. (2011) also observe that a criminal record acts as a negative credential that excludes offenders from job opportunities. Other scholars note that breaches of protection orders disproportionately disadvantage Aboriginal people (who represent around one-third of all defendants in NSW on an ADVO) and may act to entrench them in the criminal justice system (Cunneen, 2010; Douglas & Fitzgerald, 2018). Further research is therefore required to better understand the mechanism(s) by which longer ADVOs influence victim safety and, in particular, the extent to which their effectiveness depends upon police enforcement of conditions.

Several weaknesses of our data and methodology limit the interpretation of results. Firstly, we were only able to estimate an associative, rather than a causal relationship between ADVO length and subsequent offending. This is because we were unable to control for several variables which influence DV offending, including employment, alcohol use and the relationship between the victim and offender (see Hulme et al., 2019; Russell, 2012). Secondly, while we controlled for sociodemographic characteristics and prior proven court appearances, around one third of the offenders on ADVOs that we observed in this study had no prior formal DV charges. As a result, we were unable to adequately control for the specific DV-related behaviours that led to a defendant's placement on an ADVO. This is important, as prior research has found that the majority of DV offenders with no prior criminal history had been violent towards their partner in the past (Boxall et al., 2018). If defendants on longer ADVOs were more likely to be unemployed, abuse alcohol, maintain a relationship with the victim or have been involved in prior unreported DV behaviours, then we would potentially be underestimating the effectiveness of longer ADVOs in reducing DV offending. This is mitigated to some extent by our use of an event study, as this approach removes biases from unobserved confounders that were constant over time (which matching cannot adjust for), but it does not eliminate it entirely. As a result, our methodological approach estimates a conservative effect of ADVO duration on reoffending outcomes. A further limitation of our methodology is that there may be additional benefits of longer ADVOs which our study was unable to adequately capture. Previous research by Cordier et al. (2019) found that rates of DV reported by victims were higher than offending rates recorded by police (34.3% vs 28.2%, respectively). If longer ADVOs also reduce unreported DV behaviours, the benefits associated with the 2018 legislative change would be much greater than our estimates indicate. Research utilising victim survey data would be necessary to explore the impact of longer ADVOs on unreported DV and breach behaviour.

In summary, we find evidence that 24-month ADVOs are associated with greater reductions in DV offending relative to 12-month ADVOs. Furthermore, given that the drop in offending occurs in the quarters where breach rates are higher, and is no longer observed once the order expires, the increased effectiveness of longer ADVOs is likely due, at least in part, to an increased risk of punishment for proscribed behaviours. However, some caution is warranted when drawing policy inferences from our results. Firstly, it is unclear whether ADVOs longer than 24-months would deliver similar benefits as the deterrent effect of ADVOs may diminish over time. Extending the length of ADVOs also works to increase the volume of defendants subject to an ADVO at any given time. This may reduce the capacity of police to respond to ADVO breaches given limited resourcing. Finally, policymakers should be cognisant of the negative consequences arising from extended surveillance of individuals serving longer ADVOs. This is especially important for Aboriginal people given the significant contribution of breaches (including ADVO breaches) to the growth in the Aboriginal prison population over the last two decades (Weatherburn & Ramsey, 2016) and the long-term consequences of a criminal record. Future research should consider whether alternative non-legal pathways for Aboriginal people who breach or are at risk of breaching their ADVO conditions, such as behavioural change interventions, can achieve similar benefits in terms of improving victim safety.

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APPENDIX

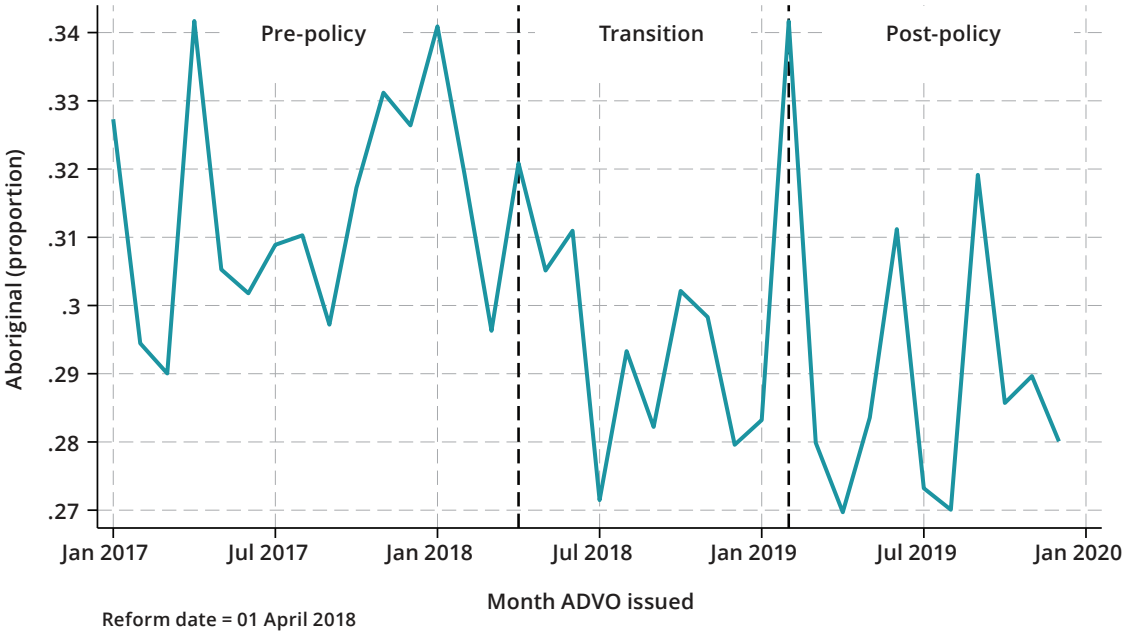
Appendix A – List of ADVO conditions

1. You must not:
    a. assault or threaten the protected person or any other person having a domestic relationship with the protected person.
    b. stalk, harass or intimidate the protected person or any other person having a domestic relationship with the protected person intentionally.
    c. recklessly destroy or damage any property that belongs to or is in the possession of the protected person or any other person having a domestic relationship with the protected person.
2. You must not approach < > or contact < > in any way, unless the contact is through a lawyer.
3. You must not approach:
    a. the school or any other place < > might go to for study,
    b. any place < > might go to for childcare, or
    c. any other place listed here < >.
4. You must not approach or be in the company of < > for at least 12 hours after drinking alcohol or taking illicit drugs.
5. You must not try to find < > except as ordered by a court.
6. You must not approach < > or contact < > in any way, unless the contact is:
    a. through a lawyer, or
    b. to attend accredited or court-approved counselling, mediation and/or conciliation, or
    c. as ordered by this or another court about contact with child/ren, or
    d. as agreed in writing between you and the parent(s) about contact with child/ren
    e. as agreed in writing between you and the parent(s) and the person with parental responsibility for the child/ren about contact with the child/ren.
7. You must not live at:
    a. the same address as <>, or
    b. any place listed here <>.
8. You must not go into:
    a. any place where <> <>, or
    b. any place where <> <>, or
    c. any place listed here <>.
9. You must not go within <> metres of:
    a. any place where <> <>, or
    b. any place where <> <>, or
    c. any place listed here <>.
10. You must not possess any firearms or prohibited weapons.

Appendix B – Roll out of ADVO length reforms

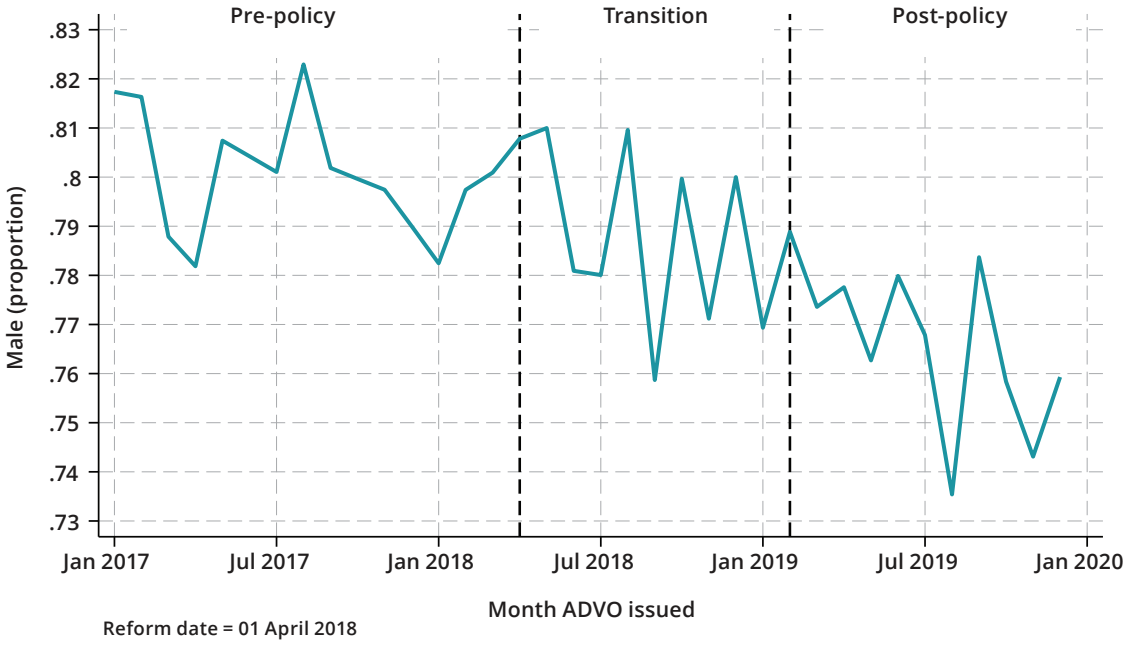
Before the change in default ADVOs issued by police, around 70% of ADVOs were 12 months long, while 20% were 24 months long (see Figure 1). After the policy was introduced, the proportion of 24-month ADVOS gradually increased to around 70% while the proportion of 12-month ADVOS slowly decreased to around 20% by February 2019. We do not implement an identification strategy which exploits variation in ADVO length around the time the reforms were introduced due to methodological concerns in possible quasi-experimental approaches. For instance, a difference-in-differences approach (see Wooldridge, 2015) was not appropriate as there was no adequate control group unaffected by the reforms.[32] On the other hand, a regression discontinuity design (RDD) approach (see Hahn et al., 2001) was not ideal because the change in default ADVO length was gradual rather than "sharp" so using observations close to the cut-off would dilute the effects of the policy. Similarly, an extension to RDD which involves removing observations during the rollout of the policy (see Burger et al., 2014) was not ideal as the characteristics of defendants issued ADVOs were different before and after the 2018 change in police practice (see Figures B1, B2 and B3). This may have been driven by separate changes in policing practices for DV offences shortly after the ADVO policy was introduced (see NSW BOCSAR (2023)).

Figure B1. Proportion of Aboriginal defendants on ADVOs around change in police practice for issuing ADVOs



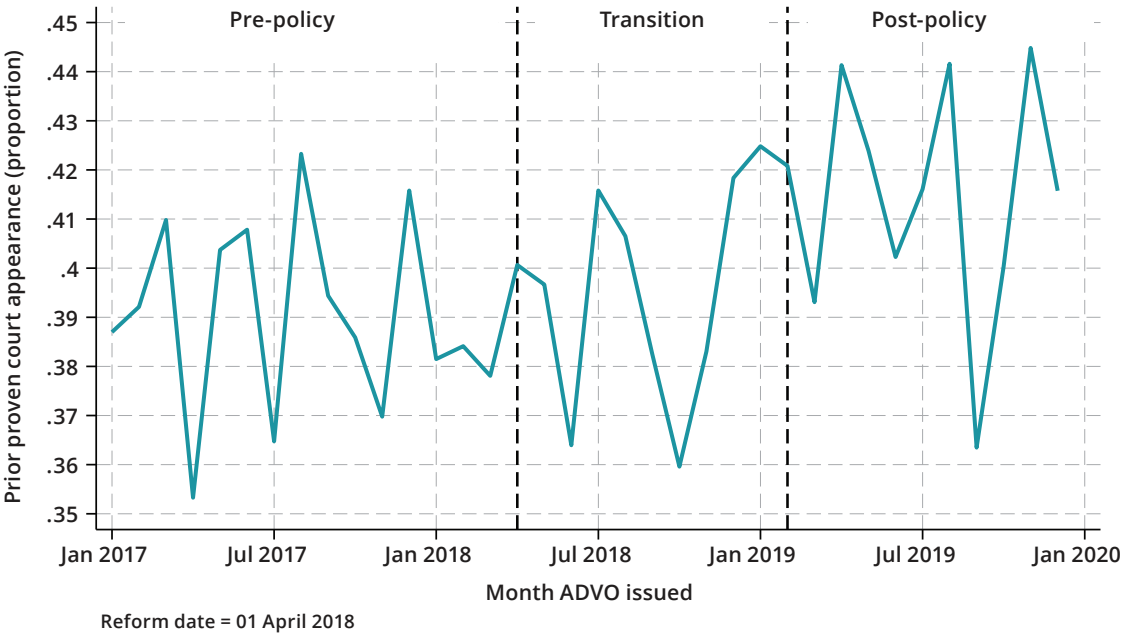
32 For example, while the reform only targeted adult ADVO defendants, we separately observe spill-over effects from the ADVO length reform which increased the average protection order duration for both juvenile defendants on ADVOs, and all defendants on Apprehended Personal Violence Orders (APVOs).

Figure B2. Proportion of Male defendants on ADVOs around change in police practice for issuing ADVOs



Reform date = 01 April 2018

Figure B3. Proportion of defendants on ADVOs with a prior proven court appearance in the last 5 years around change in police practice for issuing ADVOs



Reform date = 01 April 2018

Appendix C – Entropy balancing diagnostics

Figure C1 shows the distribution of balancing weights for defendants on 12-month ADVOs after implementing entropy balancing using the COPS sample used in our main analysis. Most weights were below 1, with a maximum weight of 4.2. This suggests that our matching analysis is not unduly influenced by a few defendants on 12-month ADVOs with large matching weights. Note that the balancing weights of all defendants on 24-month ADVOs is equal to one by construction.

Figure C1. Distribution of entropy balancing weights for offenders placed on 12-month ADVOs

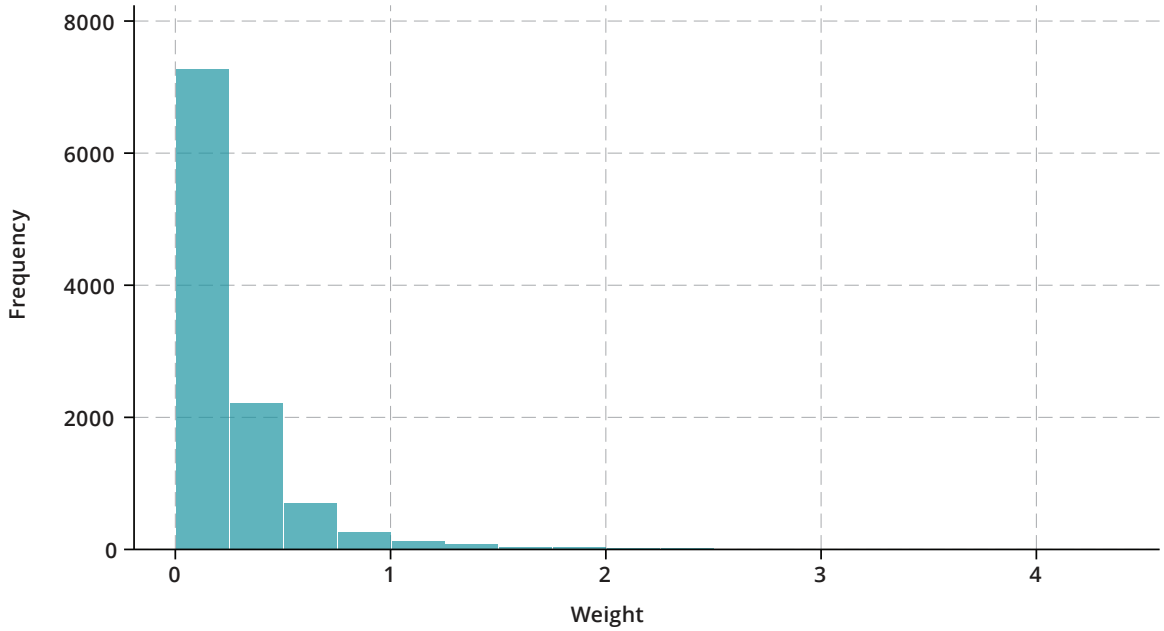
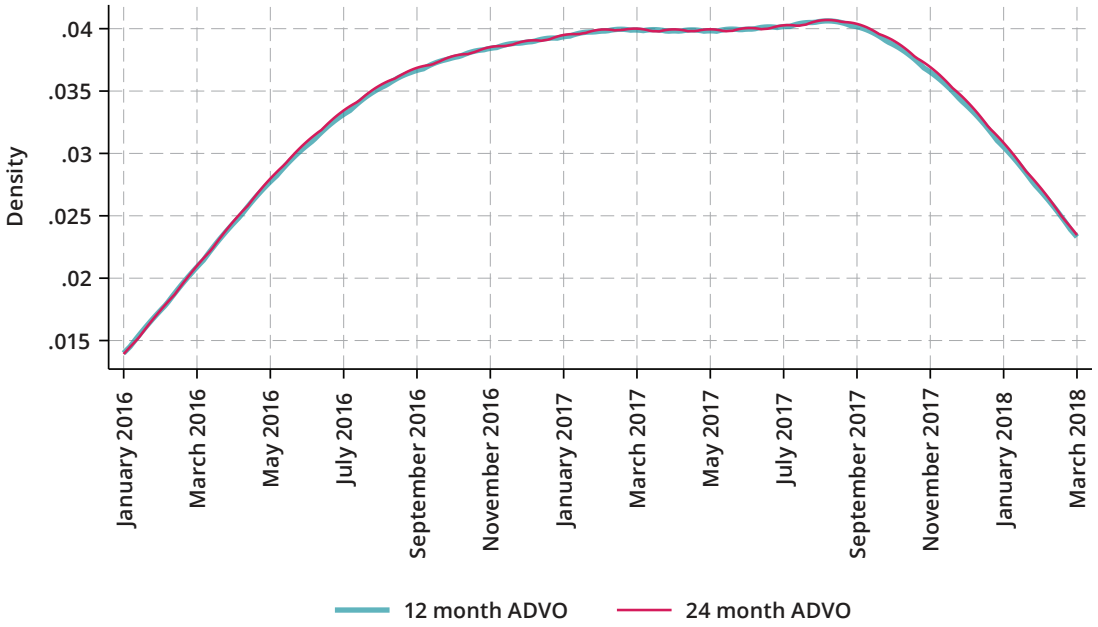


Figure C2. Proportion of balanced samples of offenders placed on 12- and 24-month ADVOs, by month and year of ADVO start date



Appendix D – Any offending

Previous research suggests that DV offenders are also involved in other forms of crime (see Weatherburn & Rahman, 2018; Dowling et al., 2021). In this section, we investigate rates of and marginal differences in any offending for defendants on 12 and 24-month ADVOs, using the same sample and matching method as our main results. Quarterly rates of any offending of defendants on 12 and 24-month ADVOs is presented in Figure D1, while Figure D2 shows marginal differences and in any offending between defendants on 12 and 24-month ADVOs and 95% confidence intervals. The general trends in any offending for those on 12 and 24-month ADVOs are analogous to those reported for DV offending in Figures 4 and 5. However, longer ADVOs only have a statistically significant impact in quarter 7, where 24-month ADVOs are associated with a 3.3 p.p decrease in any offending relative to 12-month ADVOs.

Figure D1. Quarterly rates of any offending by people placed on 12- and 24-month ADVOs

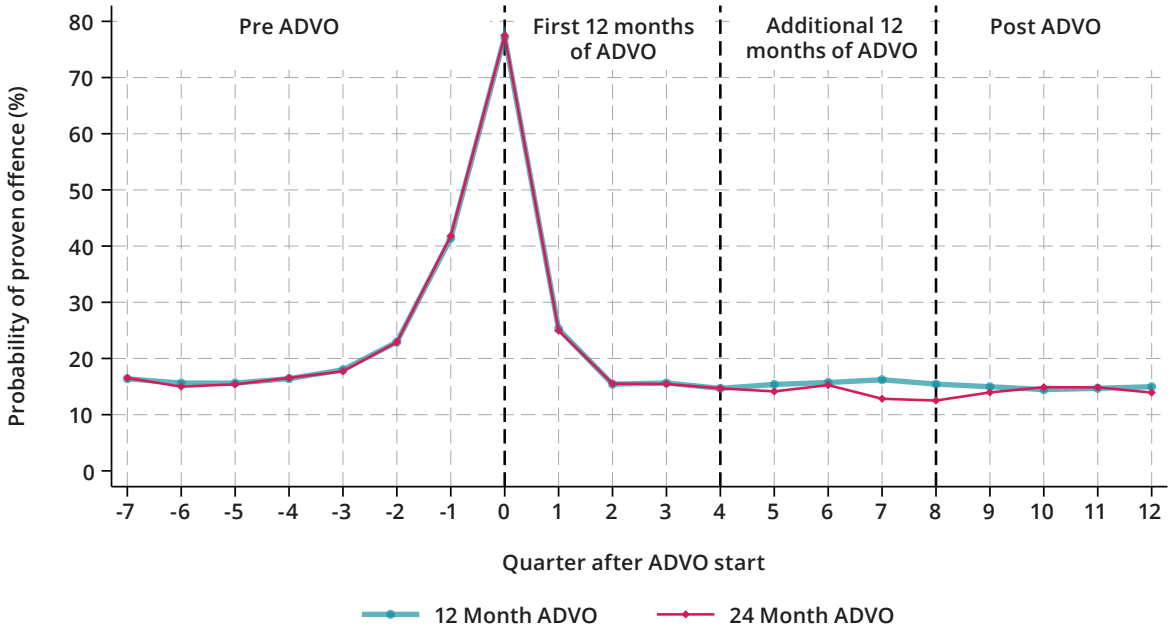
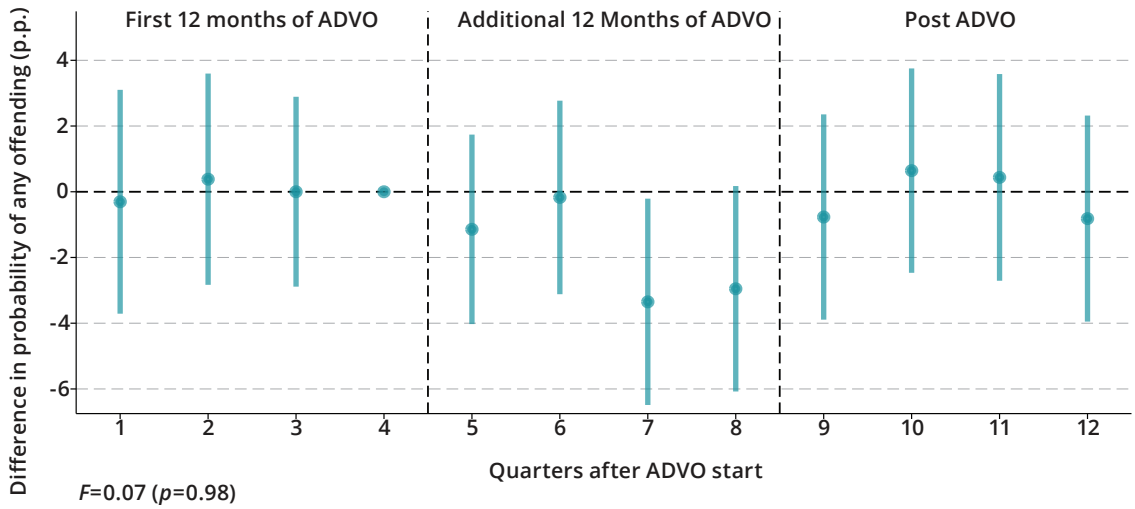


Figure D2. Event study comparing any offending outcomes in first 12, 24, and 36 months after ADVO start



Appendix E – Robustness tests

This section presents results from three robustness tests which repeat the analysis detailed in our methodology section using alternative matching methods and samples.

Appendix E1 - Matching on ADVO conditions

To test the robustness of our results, we implement an alternative matching procedure which matches not only on all of the variables listed in our data section, but also indicator variables for each ADVO condition (listed in Appendix A) which a defendant may be subject to. This allows us to control for the fact that defendants with more conditions may be more likely to breach their ADVO than those subject to fewer conditions, and that certain types of conditions may be associated with more breaches than other types of conditions. However, we could only observe data on ADVO conditions for 64% of our sample, so we match not only on condition type but also whether data on conditions are missing for a given offender. After matching on covariates (including conditions), we observe that the maximum entropy balancing weight is 3.3, which indicates that our results are not driven by outliers with excessively high matching weights. We also control for these conditions in our event study analysis.

We present quarterly rates of any offending for those on 12 and 24-month ADVOs for any offending, DV offending and ADVO breaches in Figure E1, and event study plots which compare differences in the same set of outcomes for defendants on 12 and 24-month ADVOs in Figure E2. We reach similar conclusions as our main results. Relative to 12-month ADVOs, we find evidence that 24-month ADVOs influence DV offending and ADVO breach reoffending during the additional 12-months where shorter ADVOs are no longer active. Specifically, compared to 12-month ADVOs, 24-month ADVOs are associated with an increase in ADVO breach rates by 3.5 p.p., 4.1 p.p. and 2.5 p.p. in the 5th, 6th, and 7th quarters after the start of a final ADVO respectively. Conversely, longer ADVOs were associated with lower DV offending of 1.6 p.p. and 1.9 p.p. and 2.6 p.p. in the 5th, 6th, and 7th quarters respectively.

Figure E1. Quarterly rates of any offending by people on 12- and 24-month ADVOs, by outcome variable

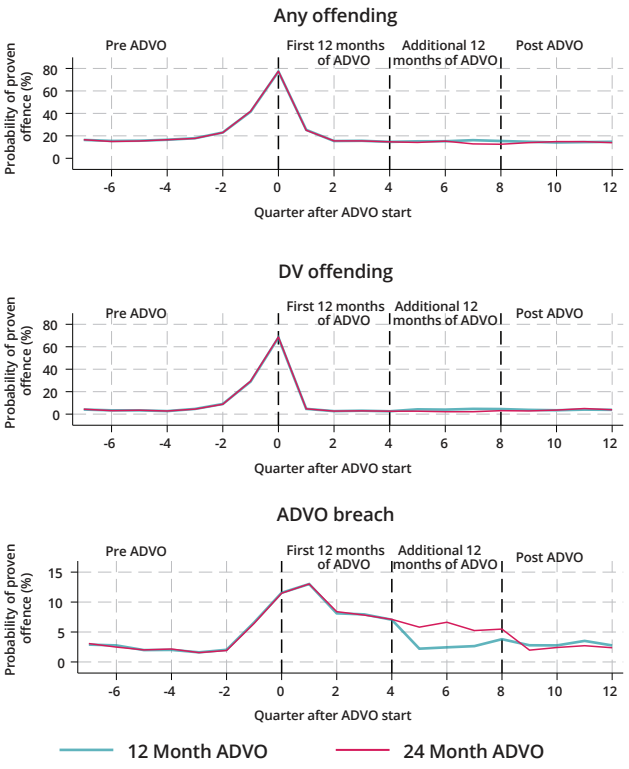
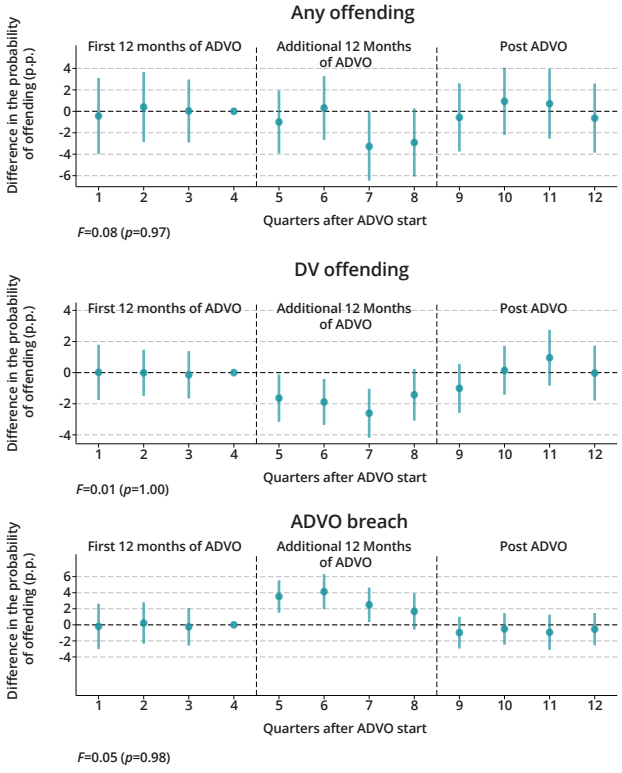


Figure E2. Event study comparing any offending outcomes in first 12, 24, and 36 months after ADVO start, by outcome variable



Appendix E2 – Sample of all offenders

As an alternative robustness test, we use the same matching procedure as presented in our main analysis but use a larger sample of all offenders placed on final 12 and 24-month ADVOs between January 2016 and April 2018 (rather than restricting our sample to those who appear in COPS). After going through the same cleaning procedure detailed in our data section, we extracted a sample of 41,738 defendants, consisting of 33,274 people on 12-month ADVOs and 8,464 people on 24-month ADVOs. While this is larger than the sample used in the main analysis, we are not able to observe several sociodemographic variables (age, sex, Aboriginality, remoteness, SEIFA quartile) for the 67% of the defendants in this sample who do not appear in COPS. Consequently, we are unable to control for these variables in our event study analysis. After matching, the maximum entropy balancing weight is 2.8, which indicates that these results are not unduly influenced by outliers with large matching weights.

We illustrate quarterly rates any offending, DV offending and ADVO breaching offending for those on 12 and 24-month ADVOs using this alternative sample in Figure E3. Event study plots which compare differences in the same outcomes for defendants on 12 and 24-month ADVOs are shown in Figure E4. These results are broadly similar to those presented in our main analysis. When compared with 12-month ADVOs, we find evidence that 24-month ADVOs influence ADVO breach offending and DV offending in the additional 12 months where only 24-month ADVOs are active. Relative to 12-month ADVOs, 24-month ADVOs are associated with higher ADVO breach rates by 3.0 p.p., 2.2 p.p., 2.1 p.p., and 1.4 p.p. in the 5th, 6th, 7th, and 8th quarters after the beginning of a finalised order, respectively. However, longer ADVOs were also associated with lower breach rates by 1.0 p.p. and 1.2 p.p. in the 9th and 10th quarters after a final ADVO began. Conversely, 24-month ADVOs are associated with decreases in DV offending by 1.7 p.p., 2.3 p.p., 2.0 p.p., and 1.1 p.p. in the 5th, 6th, 7th, and 8th quarters after the start of a final ADVO, respectively. Further, longer ADVOs are associated with 2.4 p.p., 1.6 p.p., 1.7 p.p., 1.4 p.p., and 1.5 p.p. decreases in any offending relative to shorter ADVOs, 5, 6, 7, 8 and 9 quarters after the beginning of a final ADVO, respectively. Differences between these results and out main results may be driven by the fact that this analysis cannot control for age, sex, Aboriginality, remoteness, and SEIFA quartile of offenders.

Figure E3. Quarterly rates of any offending by people on 12- and 24-month ADVOs, by outcome variable

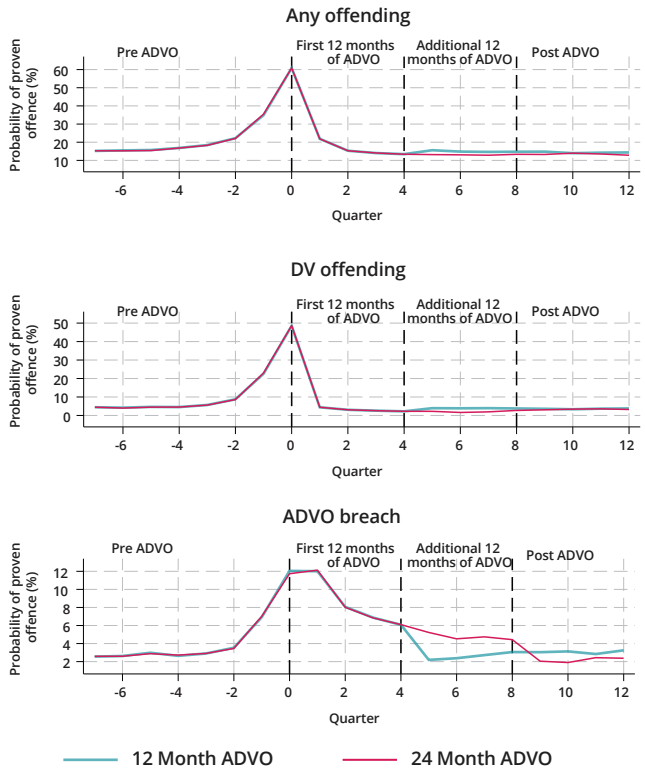
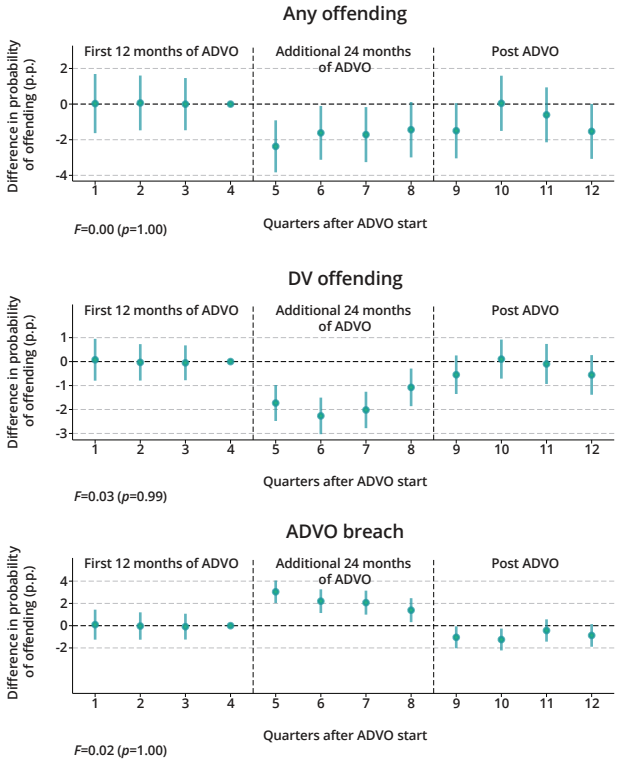


Figure E4. Event study comparing any offending outcomes in first 12, 24, and 36 months after ADVO start, by outcome variable



Appendix E3 – No pre-treatment matching

As a final robustness test, we use the same sample and set of covariates as our main analysis but match only on observable characteristics of defendants on 12 and 24-month ADVOs, and do not match on trends in pre-treatment outcomes. After matching, we found the maximum entropy balancing weight was 5.2, indicating that these results are not unduly influenced by offenders with large matching weights. Quarterly rates of any offending, DV offending and ADVO breach offending for those on 12 and 24-month ADVOs using this alternative matching method are presented in Figure E5. Event study plots which compare differences in outcomes for defendants on 12 and 24-month ADVOs are illustrated in Figure E6. Like our main results, when compared with 12-month ADVOs, 24-month ADVOs influence DV offending during the period where those on longer ADVOs are still on a protection order, while those on shorter ADVOs are not. In the 5th and 6th quarters after the beginning of a final order, we observe respective 2.9 p.p. 4.0 p.p. increases in ADVO breach offending of 24-month ADVOs, relative to 12-month ADVOs. In the 7th quarter after the beginning of a finalised order, longer ADVOs were associated with a 2.4 p.p. decrease in DV offending. This differs to our main results as the impact on longer ADVOs on DV offending is more transitory. This may be because differences in pre-treatment trends (particularly for ADVO breaches) introduces bias, which is reduced after explicitly matching on pre-treatment outcomes (see Cefalu et al., 2020).

Figure E5. Quarterly rates of any offending by people on 12- and 24-month ADVOs, by outcome variable

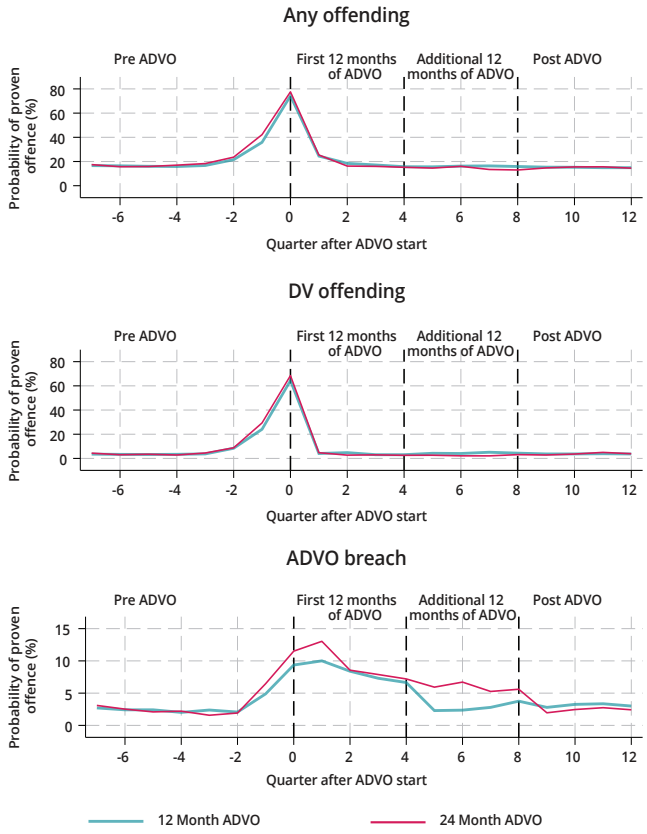
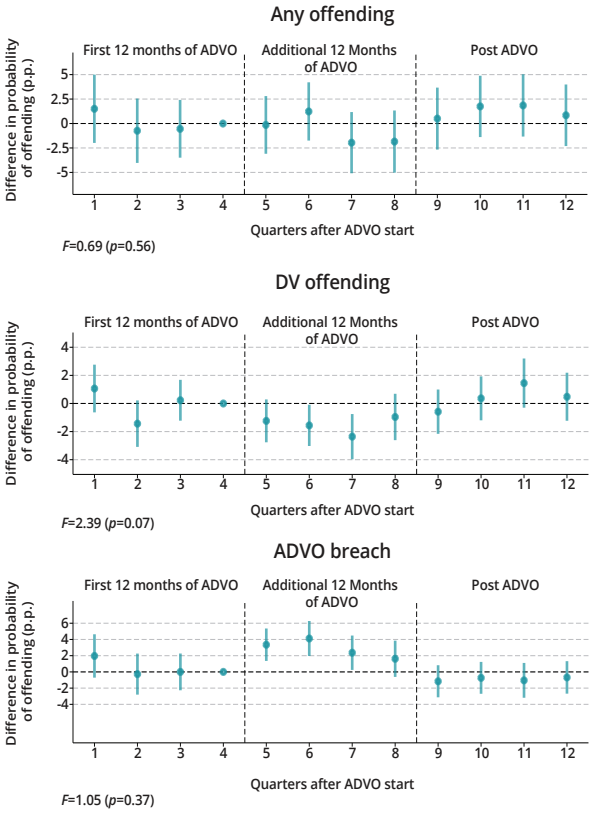


Figure E6. Event study comparing any offending outcomes in first 12, 24, and 36 months after ADVO start, by outcome variable



Appendix F – Detailed regression results

This section presents detailed results from our analysis of DV and breach offending. This includes baseline offending rates for people placed on 12-month ADVOs in each quarter. It also details event study estimates of the differences the probability of offending between people placed on 12 and 24-month ADVOs. Finally, we compute relative marginal effects representing the difference in offending as a percentage of baseline reoffending rates.

Table F1. Summary of detailed regression results

	Quarters post ADVO start								
	4	5	6	7	8	9	10	11	12
DV offending									
Baseline offending rate of those on 12-month ADVOs (%)	2.57	4.40	4.18	5.17	4.54	3.90	3.65	4.14	3.90
Difference in offending, 24 vs. 12-month ADVOs (p.p.)	0.00	-1.78	-2.04	-3.06	-1.42	-1.09	-0.07	0.85	-0.05
p-value of difference	1.00	0.02	0.01	0.00	0.08	0.17	0.94	0.35	0.95
Relative marginal difference in offending (%)	0.00	-40.57	-48.79	-59.28	-31.37	-28.04	-1.84	20.50	-1.32
ADVO breaching									
Baseline offending rate of those on 12-month ADVOs (%)	7.10	2.38	2.54	2.91	3.85	2.89	3.08	3.46	3.01
Difference in offending, 24 vs. 12-month ADVOs (p.p.)	0.00	3.43	4.10	2.30	1.67	-1.01	-0.80	-0.81	-0.70
p-value of difference	1.00	0.00	0.00	0.04	0.15	0.32	0.43	0.46	0.49
Relative marginal difference in offending (%)	0.00	143.91	161.33	78.89	43.32	-34.86	-25.93	-23.45	-23.21

Note. This table present estimates corresponding to the main results from our paper. Baseline offending rates refer to the percentage of offenders placed on a 12-month ADVO who offend in each respective quarter after an ADVO is issued. Rates correspond to those in Figures 5 and 7 for DV and breaching, respectively. Percentage point differences in offending refer to event study estimates detailed in the methods section, and correspond to those illustrated in Figures 6 and 8 for DV and breach offending, respectively. *p*-values relate to event study estimates. Relative marginal differences are calculated as percentage changes from the baseline.

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