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The impact of the 'What's Your Plan?' program on ADVO breaches and domestic violence

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AIM

The aim of this study is to estimate the causal impact of the `What's Your Plan' (WYP) program on rates of Domestic Violence (DV) related offending by Aboriginal defendants issued an Apprehended Domestic Violence Order (ADVO).

METHOD

The 'What's Your Plan' program was only available to Aboriginal defendants receiving a finalised ADVO between March 2017 and October 2019. The availability of this program was quasi-randomised, only being offered to defendants on alternate weeks. If this allocation of the program was 'as good as random', this allows us to estimate the causal impact of 'What's your Plan' on rates of DV related offending.

Our analysis considered two offence types as primary outcome measures: any new breach ADVO offence and any new proven DV charge. For both these offence types, the likelihood of the offence occurring within 3, 6, or 12 months and the number of days until the offence occurred (if ever) were considered. We estimated the average treatment effect for two groups of interest:

- all defendants allocated to receive the program regardless of whether they ultimately received the program (an intention-to-treat estimate);
- all defendants who accepted and received the program (a treatment-on-treated estimate).

For the intention-to-treat analysis, the impact on the likelihood of the offence occurring was estimated using a logistic regression model, and the impact on the number of days until the offence occurred (if ever) was estimated using a tobit regression model. For the treatment-on-treated analysis, the analogous instrumental variable model was used for each outcome measure.

RESULTS

We find no evidence for any causal impact of the 'What's Your Plan' program on breaches of ADVOs or DV-related charges. For each of our outcome measures, we estimate small differences between those that were allocated to receive WYP and those that were not. Differences were larger when comparing those that were offered and received the WYP program with those not allocated to the program but less precisely estimated. None of these differences were statistically significant at conventional levels.

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Using an observation period of 12 months after trial entry, we observed a 1.6 percentage point (p.p) reduction in ADVO breaches and a 1.2 p.p. reduction in DV related offending for those allocated to receive WYP. Time to first new breach and DV charge also increased by 13 and 10 days, respectively. Three and 6-month differences were smaller and/or in the opposite direction than expected. None of these differences were statistically significant.

Comparing those that were offered and received the program to those receiving business-as-usual, again using an observation period of 12 months after trial entry, we observed a 4.1 p.p. reduction in ADVO breaches and a 3.1 p.p. reduction in DV related offending. Time to first new ADVO breach and DV charge also increased by 35 and 24 days, respectively. Again, differences at 3 and 6 months were smaller and/or in the opposite direction than expected. Again, none of these differences were statistically signficant.

CONCLUSION

This evaluation finds no evidence that the 'What's Your Plan' program is sufficient to reduce DV-related offending for Aboriginal defendants. This does not imply that a program like WYP aimed at enabling self-directed behavioural changes and overcoming behavioural barriers would not be effective within a suite of other programs, supports and resources.

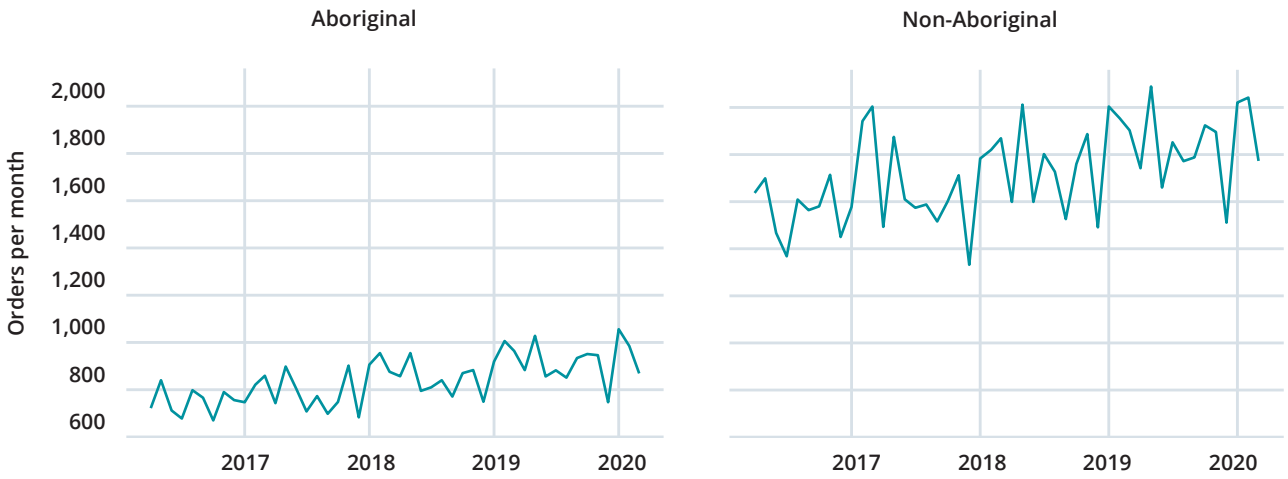
KEYWORDS

Domestic violence | Apprehended violence orders (AVO) | Program evaluation | Aboriginal over-representation | Recividism / Re-offending | Behavioural insights

INTRODUCTION

Apprehended Domestic Violence Orders (ADVOs) are civil court orders designed to protect individuals from ongoing domestic violence and prevent re-victimisation. The number of ADVOs granted has steadily increased in NSW in the last four years, with 33,278 final ADVOS issued between April 2019 and March 2020, compared with 28,903 between April 2016 and March 2017, an increase of 15%.[1] This increase has been even more rapid for Aboriginal people, with the number of final ADVOs issued across the same period increasing from 9,159 to 10,988 (or 20%).

Figure 1. ADVOs granted per month from April 2016 – March 2020, by Aboriginality of defendant



While the evidence suggests that ADVOs are associated with significant reductions in severe violence, breaches remain unacceptably high (Dowling et al., 2018). In 2016 and 2017, the NSW Department of Communities and Justice's Aboriginal Services Unit (ASU) and the Department of Premier and Cabinet's Behavioural Insights Unit (BIU) co-developed 'What's Your Plan' (WYP); a brief behavioural intervention designed to increase Aboriginal defendants' compliance with ADVOs.

This report estimates the causal impact of the WYP program on ADVO breach rates and domestic violence related offending based on a trial of this program with Aboriginal defendants. We can identify the causal impact of the WYP program because defendants were quasi-randomised to the program using an 'on week, off week' alternating allocation process.

The effectiveness of protection orders

In NSW, any person who is, or has been, the victim of physical assault, threats of physical harm, stalking, intimidation or harassment in a domestic relationship and has a reasonable fear to believe that this behaviour will continue, can apply to the Local Court for an ADVO. The *Crimes (Domestic and Personal Violence) Act* 2007 (NSW) recognises a wide range of relationships as 'domestic'. The defendant could be currently or previously:

- a spouse of the person seeking protection;
- a de facto partner;
- in an intimate relationship with the protected person;
- living in the same household as the protected person;

1 This increase continued during the COVID-19 pandemic, with 34,257 final ADVOs issued between April 2020 and March 2021, but may not be comparable due to the change in context.

- a long-term resident in the same residential facility as the protected person;
- dependent on the paid or unpaid care of the protected person;
- part of the protected person's Indigenous kinship system or extended family; and/or
- a relative of the protected person (including step- and in-law relationships).

If granted, the defendant named in the order must comply with three mandatory conditions: not to assault, harass or threaten the protected person; not to intimidate the protected person; and not to stalk the protected person. If these, or any other conditions set down by the court[2] are breached, the defendant can be charged with a criminal offence.

The existing research on protection orders suggests that they are effective in reducing violence, abuse, harassment and re-victimisation. Holt et al. (2003) interviewed 253 women in the USA who were granted a protection order and 195 women who had contact with police for a domestic violence incident but did not obtain a protection order. Each participant completed a baseline interview, and then two follow-up interviews at approximately 1 month and 9 months after obtaining the protection order or contacting the police. Women granted a protection order were found to be less likely to experience domestic violence, particularly between the first and second follow-up interviews.

Kothari et al. (2012) report similar results from a study undertaken in Michigan, USA. Using a propensity score matching approach, they compared rates of police recorded domestic violence incidents for women with protection orders with a group of "similar" women who had experienced violence but did not seek a protection order. Rates of violence were measured before, during and after the order was granted, as an annualised rate to allow for different lengths of follow-up time. Kothari et al. (2012) found that those with protective orders had higher police incident rates before the protective order was obtained, but similar rates whilst the order was in effect and after the order had expired.

The results from the US are echoed by a study conducted by Kelly et al. (2013) in the UK, which examined the impact of introducing a new type of protective order, domestic violence protection orders, in a 15-month pilot. These protection orders were again associated with fewer incidents of domestic violence within approximately one year of the index event, compared to similar cases dealt with by arrest but where "no further action" ensued.

Evidence from NSW also suggests that protection orders are effective. Trimboli and Bonney (1997) used surveys and interviews to assess the prevalence of domestic violence victimisation across 115 individuals who received an Apprehended Violence Order shortly after the scheme commenced in NSW. The interviews took place at the time the orders were granted and then one, three and six months after the orders commenced. More than 90% of individuals reported that the protection order had produced benefits. The authors concluded that there was a reduction in physical assaults, threats, stalking and other forms of intimidation and harassment (pp. viii). These results were replicated nearly 20 years later by Trimboli (2014). She interviewed 147 individuals before and after they obtained a protection order in one NSW Local Court and found that six domestic violence behaviours (stalking, verbal abuse, contact others, intimidation, physical assault and threats of physical assault) decreased sharply whilst the order was in effect.

McFarlane et al. (2004) report somewhat conflicting results. They interviewed 81 women who applied for, and were granted, a protection order and 69 who applied but were not granted a protection order, about their experiences of violence at 3, 6, 12 and 18 months after the protection order decision. The authors found that all women who applied for a protection order experienced lower levels of violence irrespective of whether they were granted the order. They suggest that there may be significant differences between those that apply for a protection order and those who experience violence but do not apply for

2 The court may also specify any other conditions that are deemed necessary to protect the person(s). This can include restrictions on: where the defendant may reside; who the defendant is able to contact; and how close the defendant can be from specified individuals (after the consumption of drugs and alcohol).

protection. If these differences are not accounted for then observational studies may be over-estimating the impact of the protection order itself.

Overall the evidence suggests that protection orders are effective in reducing violence, abuse and harassment, although the results from some studies showing a positive effect may be confounded by unobserved differences between women who are granted a protection order and those who are not. Importantly for our study, we were unable to find any research examining the extent to which protection orders are effective for vulnerable groups, such as Aboriginal people. This is an area where further research is clearly needed.

Strategies for increasing compliance with ADVOs

While the evidence suggests that protection orders are effective in reducing the incidence of domestic violence, they do not eliminate the risk to victims entirely. Poynton et al. (2016) estimate that around one in five ADVOs are breached in NSW, with most of these breaches occurring within three months of the order being granted. Aboriginal defendants were found to breach ADVOs at a higher rate than non-Aboriginal defendants (24.6% vs 16.8% within 12 months), unadjusted for any differences in other characteristics. Males and younger defendants were also found to be more likely to breach their order sooner than other defendants. As highlighted by Poynton et al. (2016), the actual breach rate is likely to be significantly higher than their estimates suggest given that only breaches reported to or detected by police were included in the analysis. Given the evidence suggesting that Aboriginal defendants are more likely to be imprisoned for breaching an ADVO (Napier, Poynton, & Fitzgerald, 2015; Nancarrow, 2016; Thorburn & Weatherburn, 2018), this disparity likely has a range of serious flow-on impacts for Aboriginal people.

There is very little research on interventions that aim to enhance compliance with protection orders. One of the few studies in this area was undertaken by Trimboli (2014). She evaluated the impact of a legal advice service provided to defendants in ADVO proceedings on the frequency with which ADVOs were breached. Examining victims' experiences of proscribed behaviours before and after an ADVO was issued, Trimboli (2014) found that breach rates reduced by the same amount regardless of whether the legal service was made available to the defendant. This suggests that a lack of understanding of the consequences of breaching an ADVO is not the critical factor driving breach behaviour. The dearth of research in this area has led some to consider whether interventions shown to be effective in other areas of social policy could be adapted and applied in this context.

Mental Contrasting and Implementation Intentions (MCII) is a combination of two interventions from the psychology literature aimed at closing the 'intention-action' gap. MCII is designed to help those who have an existing goal but are having difficulty committing to their goal and striving towards that goal (Oettingen & Gollwitzer, 2010). A Mental Contrasting (MC) intervention prompts individuals to imagine a desired future and contrasting that future with the present reality. This is intended to prompt the individual to consider the actions that are required to realise the desired future, energise them to take action, and strengthen their goal commitment. The Implementation Intentions (II) intervention prompts individuals to develop an "if-then" plan that prompts the individual to elaborate on what actions would need to take place to meet their goal. This helps individuals to get started and then stay focused on their goal in the face of obstacles. These interventions are seen to be complementary and are often delivered together to help people with goal attainment.

The intention-action gap is a problem that exists across a broad range of domains, leading to MCII being trialled in a number of disparate settings. Cross and Sheffield (2019) provide a systematic review and meta-analysis of the efficacy of MCII in changing health behaviours, such as smoking and unhealthy snacking. Based on a review of 12 studies they conclude that MCII can modify behaviour, at least in the short-term (i.e. for up to three months). There is also evidence to suggest that this intervention works to reduce alcohol consumption (Wittleder et al., 2019), improve academic performance in children (Duckworth et al., 2013) and help people regulate their anger (Gallo, 2018).

While these results are promising, there is no research to our knowledge that has considered whether MCII strategies are effective for improving compliance with court orders or indeed any research examining the use of MCII strategies within a criminal justice setting.[3] If MCII strategies were shown to be successful in reducing the aggregate rate of court order breaches, this would imply that the 'intention-action' gap may be a critical barrier for ADVO compliance. However, there are several important differences between the context in which an ADVO is issued and the contexts in which MCII has been successfully trialled that may reduce or nullify the effectiveness of the intervention, including the lack of agency in receiving an ADVO (and thus in forming a goal to comply), the long-term nature of the outcome being measured and the outcome being dependent upon the other person in the relationship (rather than relating to just an individual's own goals and desired future).

The 'What's Your Plan?' program

Background

In 2016 and 2017, the NSW Department of Communities and Justice's Aboriginal Services Unit (ASU) and the Department of Premier and Cabinet's Behavioural Insights Unit (BIU) co-developed 'What's Your Plan' (WYP). The core element of the WYP program is its use of two self-regulatory behaviour change strategies:

1. Mental contrasting (MC) - Mental contrasting is a technique where individuals compare a desired future with their current reality (Oettingen & Gollwitzer, 2010). For WYP, this involves defendants imagining a positive future arising from the benefit of complying (versus not complying) with their order.
2. Implementation intentions (II) - Implementation intentions foster goal implementation by having individuals create an "if-then" plan in which a specific cue triggers a desired behaviour (Oettingen & Gollwitzer, 2010). For WYP, this involves defendants identifying an obstacle that could prevent them from complying with their ADVO and then developing a suitable plan to overcome this obstacle.

WYP was designed as a "process enhancement" for Aboriginal Client and Community Support Officers' (ACCSOs) existing frontline support work, providing a specific structure for their interaction with DV defendants. WYP underwent an extensive development and co-design process with ASU to clarify its operational requirements with the object of creating a program that would be considered culturally appropriate by ACCSOs, defendants and their communities. Co-design began with group consultation between ASU, Courts and Tribunal Services and other members of the WYP governance group (including NSW Police, Aboriginal Legal Services, and NSW Legal Aid) and invited representatives from DV program providers. Workshops were held with ACCSOs to refine the format and wording of the tool and planning sessions.

WYP was piloted in several NSW Local Courts from March 2017 to May 2017 and was rolled out to other courts from June to September 2017. One WYP ACCSO was appointed in each ASU region to deliver WYP, support other ACCSOs to implement WYP, and collate their region's data. Staff completed a one-day training session prior to delivering WYP. During this time, Quality Assurance (QA) procedures and an online program database were developed. Dedicated QA roles were also established. A QA Coordinator was appointed to train, observe, and coach ACCSOs in their implementation of WYP (including delivery of WYP sessions, data recording, and adherence to trial procedures). As part of this process, the Bureau of Crime Statistics and Research (BOCSAR) was engaged to conduct a process and outcome evaluation of WYP (Nelson (2018), and this document respectively). Further detail on the implementation and design decisions made for this program are provided by Nelson (2018).

3 The majority of interventions motivated from the psychology literature have applied exercises and techniques from cognitive behavioural therapy (e.g. see Lipsey, Landenberger, and Wilson (2007) for a meta-analysis on re-offending). MCII is drawn from the self-regulation literature, and the results from interventions from the cognitive behavioural therapy literature are not likely to be indicative of results from an MCII intervention.

We worked collaboratively with the NSW ASU and the NSW BIU to design the most rigorous evaluation design possible given the practical constraints on the ACCSOs to deliver the program effectively.[4] This resulted in the ACCSOs alternating the weeks in which the WYP program was delivered, so the effectiveness of the WYP program could be compared against a realistic benchmark (e.g. services that would otherwise be available).

The WYP program

The WYP program consisted of two key components:

1. The planning session, which includes the delivery of the MCII intervention;
2. Follow-up reminders, which includes a follow-up call and co-designed SMS prompts.

ACCSOs were emailed court lists by court registry staff one week before the expected appearance date to identify individuals eligible for the trial. While court lists identify the nature of each matter, they provide no information about the defendant's Aboriginality. This meant that ACCSOs had to rely on other sources of information, such as their personal knowledge of the defendant, surnames of families in the area, or even a general visual assessment to determine if the defendant was likely to be Aboriginal. Aboriginality was often unknown prior to meeting the defendant, particularly in high volume courts, and was confirmed once the ACCSO was able to meet with them in person. Nelson (2018) estimates that approximately a third (32%) of all eligible defendants were identified when WYP was implemented, but the identification rate varied between courts, ranging from 10-70%.

Once identified, ACCSOs approached eligible defendants in court following their court appearance and invited them to participate in a 30 minute session to talk through the conditions of their order and develop a plan to promote compliance. Eligible defendants were informed that the meeting was voluntary and further information would be provided on request.

The initial planning phase of the session took roughly five minutes, and was spent discussing:

- the conditions of the ADVO and the implications of the order for the defendant's daily life;
- reasons and motivation for compliance; identifying obstacles and barriers to compliance; and creating an action plan for dealing with obstacles (e.g. the MCII intervention).

A summary of the action plan was then recorded on paper folded to the size of a business card containing:

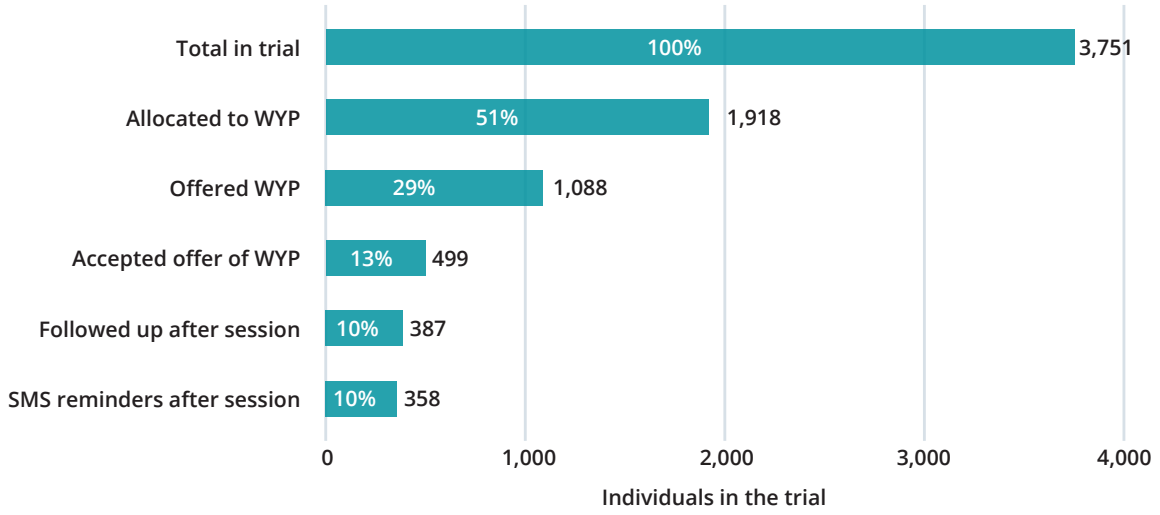
- the defendant's goals;
- their motivation;
- an if-then plan and concrete actions the defendant could take in situations where there was a risk of breaching the order.

The ACCSO and participant also agreed on a time for a follow-up call (to occur 2 to 7 days later), where the ACCSO would remind the participant about their plan and adapt it as necessary. Some participants also co-designed SMS prompts to be sent by the ACCSO to the defendant at agreed times and frequencies in order to support the defendant at times likely to be high risk.

4 As part of the co-design process with ACCSOs, it was determined that trying to randomise each individual to receive WYP would ultimately add too much confusion and work for the ACCSOs during the trial period.

The graph below shows the number of individuals in the trial and the proportion who received each component of the intervention. A total of 3,751 individuals took part in the trial, with individuals allocated to either receive WYP or receive business-as-usual (BAU) services on alternating weeks, using the date for their first appearance at court for their ADVO. Using this allocation, 1,918 people were allocated to receive WYP (51% of those in the trial), 1,088 (57% of those allocated) were offered the program and 499 (46% of those offered, 26% of those allocated) ultimately accepted the offer and received WYP. The majority (72%) of those who received WYP also received the follow-up call and SMS reminders.

Figure 2. Number and percentage of participants in the trial that received each component of the treatment



There were a number of logistical barriers that prevented some defendants who were allocated to WYP from ultimately being offered the program. ACCSOs had a limited amount of time to find and engage each client before the client left the courthouse, and if the individual accepted the offer, the WYP program typically took at least half an hour to deliver. This, along with a high existing workload, meant that ACCSOs were unable to meet the demand for the program. ACCSOs did not routinely ask why individuals declined the program, but interviews with stakeholders suggested that some defendants were reluctant to engage in any program, and were particularly reluctant to speak about domestic violence in a setting where the person in need of protection was potentially nearby (Nelson, 2018).

The current study

The 'What's Your Plan' intervention was only available to individuals receiving a finalised ADVO in alternating weeks who were identified as Aboriginal by the ACCSOs. This quasi-random assignment to treatment is the critical element of the WYP trial from an evaluation perspective as it allows for causal estimates of the treatment on offending to be generated if allocation is "as good as random" (Fisher, 1935; Pearl, 2009). In this report we investigate both the average impact of WYP on those who were allocated to receive the program (i.e. intention-to-treat; 1,918 individuals in our study) as well as the average impact on those who were offered and received the program (i.e. treatment-on-treated; 499 individuals in our study).

We are primarily interested in the effect of participation on breaches of ADVOs, as measured by whether a breach occurred within a specified period (three, six or twelve months), and the number of days until the first breach (if there is one). However, we also present evidence on the impact of WYP on domestic violence related offending behaviour within three, six and twelve months of the initial court appearance.[5]

5 Appendix A provides estimates using a range of cut-off values to demonstrate that the results are robust across time.

METHOD

Data source

We have used two sources of data for this evaluation:

1. Data on eligibility for and participation in the WYP program provided to BOCSAR by ASU;
2. Data on offending outcomes drawn from the BOCSAR reoffending database (ROD).

WYP program data were collected by 57 Aboriginal Client and Community Support Officers (ACCSOs) working across 55 courts during the trial period.[6] These data identified all ADVO defendants eligible to participate in the trial, whether they were allocated to receive the treatment, and if so, whether they attended the WYP session and participated in the planning sessions. Data was originally recorded in individual spreadsheets and collated on request, but this was transitioned to a purpose-built website in mid-October 2017 (Nelson, 2018).

Records for 3,838 individuals were available in the WYP program dataset. Of these, 3,756 (97.9%) were successfully matched to the BOCSAR Re-Offending Database (ROD) using the unique Criminal Names Index (CNI) as a person identifier. Four individuals were duplicated in the original WYP program data; 3 were allocated multiple times to WYP and one was allocated to both receive and not receive WYP. The individual allocated to both conditions was dropped from all analyses. For the other three cases, the first episode of WYP participation was considered the index event.

This leaves us with 3,751 individuals in our dataset; 1,833 individuals in the business-as-usual (BAU) condition and 1,918 allocated to receive WYP.

Sample

The sample for this study includes all individuals identified by ACCSOs during the trial period (March 2017 – October 2019) as meeting the inclusion criteria for the trial, namely:

- non-incarcerated adult;
- identified as Aboriginal;
- issued an interim or final ADVO at a WYP court during the trial period.

For this analysis, each of the 3,751 rows in the data corresponds to a unique individual who participated in the trial. We have extracted finalisations up until April 2020, allowing time for breaches and offences to be finalised in court to avoid any differences in the offending environment during the COVID-19 pandemic. There is some attrition in the sample used to estimate treatment effects at the six- and twelve-month marks because not all individuals had sufficient time after the program for these outcomes to be measured. We observe three-month outcomes for all individuals (1,833 in BAU and 1,918 in WYP), but we do not observe six-month outcomes for 36 individuals (17 in BAU, 19 in WYP) and twelve-month outcomes for 812 individuals (431 in BAU and 381 in WYP).[7]

6 WYP was implemented in Albury, Armidale, Ballina, Batemans Bay, Bathurst, Blacktown, Bourke, Brewarrina, Broken Hill, Campbelltown, Casino, Coffs Harbour, Condobolin, Coonabarabran, Coonamble, Cowra, Downing Centre, Dubbo, Forbes, Forster, Gilgandra, Gosford, Grafton, Griffith, Kempsey, Lake Cargelligo, Leeton, Lismore, Macksville, Maclean, Moree, Moruya, Mt Druitt, Narooma, Narrandera, Narromine, Newcastle, Nowra, Orange, Parkes, Parramatta, Penrith, Port Kembla, Raymond Terrace, Tamworth, Taree, Toronto, Wagga Wagga, Walgett, Wellington, Wentworth, Wilcannia, Wollongong and Wyong local courts.

7 For all individuals where we do not observe their six month outcome, we also do not observe their twelve month outcome.

Variables

Outcome variables

The two pre-specified primary outcomes for the WYP program are:

1. Number of days between program entry (ADVO issue date) and first new breach ADVO: defined using the appearance date and the date on which the first breach occurred. If there was no breach offence, they were given the maximum number of days before the cut-off period;[8]
2. Number of days between program entry (ADVO issue date) and first new DV charge of any kind: defined using the difference between the appearance date and the offence date of the DV offence. If there was no DV related offence, they were given the maximum number of days before the cut-off period.[9]

We also estimate the impact of receiving the WYP program on two binary measures of these outcomes:

1. Whether they breached an ADVO: a binary variable defined by whether they were found guilty of breaching an Apprehended Domestic Violence Order;[10]
2. Whether they committed a DV offence: a binary variable defined by whether they were found guilty of any offence marked as DV related.[11]

All outcomes were analysed at three, six and twelve months after ADVO issue date.

Independent variables

For this analysis, we use two treatment indicators to estimate the impact of the WYP program:

- Allocated: a binary variable defined by whether the individual entered the trial in a treatment week or a business-as-usual week;
- Received: a binary variable defined by whether the individual is recorded as receiving the intervention. As detailed on p.8, 1,088 of the 1,918 individuals allocated to receive WYP were offered the intervention (57%), and 499 individuals were offered and accepted the WYP program (26% of those that were allocated to receive WYP).

We also include a range of defendant characteristics as controls in the analysis. All data items are drawn from the ROD database, except for "Region" which comes from ASU.

- Gender: a binary variable coded 0 for males and 1 for females.
- Region: a factor variable that can take the values Metro, NorthWest, Northern, Southern or Western, giving the location of the person's residence.
- Age: a continuous variable indicating the age of defendant at date of data extraction (i.e. 1/4/2020).
- Remoteness: A factor variable that can take the values Major cities, Inner regional, Outer regional, Remote, Very remote, indicating the Australian Bureau of Statistics (ABS) standard remoteness structure of the persons residence.
- Prior offending information:
  - Whether the individual breached an ADVO order in the last five years (TRUE or FALSE);
  - Whether the individual committed a DV related offence in the last five years (TRUE or FALSE);
  - The number of proven offences in the last five years (positive integer, 0-20).

8 This analysis was also conducted using the number of days when not in custody ('free days') rather than the days between the ADVO issue date and the breach date. We found no qualitative differences using this measure.
9 This analysis was also conducted using only serious violence related to DV. We found no qualitative differences using this measure.
10 Any offence where the lawpart was 65020, and they were found guilty of that offence
11 Whether the offence was flagged as DV related, and whether they were found guilty of the offence.

Empirical strategy

Estimating the average impact of WYP on those who were offered the program ("intention-to-treat")

Eligible defendants were quasi-randomised into WYP, with randomisation being determined by the week the defendant was issued the ADVO. Each court alternated between WYP and "business-as-usual" (BAU) weeks. For example, defendants receiving a finalised ADVO order in one of the ACCSO courts during a specified week (e.g. October 2 - 6) were assigned to the WYP group and defendants issued an ADVO in the following week (e.g. October 9 - 13) were assigned to the control group. This means that whether the individual entered the trial in a WYP week (or not) is credibly unconfounded with respect to the outcome variables being measured. Treatment status was recorded by the ACCSOs directly to minimise any possibility of measurement error.

The unconfoundedness of the alternating week allocation for WYP is the foundation of the identification strategy for this evaluation. As we have full information on how entry into the WYP program was determined, we can rule out reverse causality and/or the presence of unobserved heterogeneity (such as selection into WYP by a less risky cohort).

Any potential threat to the identification strategy would be through a factor that is correlated with the alternating structure of our quasi-randomisation and that has a significant impact on breaches and re-offending. For example, one possible confound would be through the ACCSOs systematically selecting defendants of differing risk during the on and off weeks as the nature of the intervention meant that they could not be blinded to the treatment. We have no indication that this occurred during the trial (Nelson, 2018).

We believe that the most plausible threat to our identification strategy is spill-over through habit / persistence in the behaviour of the ACCSOs between weeks which would lead us to underestimate the impact of the intervention. As noted above, treatment status is recorded by the ACCSOs, leaving open the possibility that some participants received the intervention in 'business-as-usual' weeks or that some modified version of the MCII intervention was unintentionally adopted into standard practice. Based on discussions with the ASU, we do not believe this risk poses a significant threat to our analysis.

Finally, it is unlikely that there are any violations of the stable unit treatment value assumption that underlies our analysis.[12] The individuals develop their own WYP plans, and outcomes are determined independently of other defendants in the vast majority of cases.

This means that we can estimate the average causal impact of being offered WYP on reoffending in our sample by simply comparing the average difference in the outcome(s) between those who entered the trial in a WYP week and those who entered the trial in the BAU week.

Estimating the average impact of WYP on those who received the program ('treatment-on-treated')

We use an instrumental variable approach to estimate the impact of WYP on the offending rates of those who agreed to and received the intervention (approximately 35% of those offered WYP).[13] For this analysis, our instrumental variable is simply whether the week the participant entered the trial was a treatment week since this determined entry into the program. Our treatment variable is whether the individual both agreed to and received the WYP program.

12 The stable unit treatment value assumption involves assuming that treatments applied to one unit do not affect the outcome for another unit.
13 This approach scales the change in outcome for those allocated to WYP by the proportion of people who received the program. If we were to compare outcomes for those who received WYP and those that were allocated in business-as-usual directly, the difference in outcomes could be driven by unobserved differences between those who accepted the WYP program and those that did not.

To be considered valid the instrument must satisfy four conditions.

1. Ignorability of the instrument: The quasi-randomisation ensures that the instrument is unconfounded with respect to both the treatment and the outcome. This is analogous to the argument given for the identification strategy for the 'intention-to-treat' estimate in the section above.
2. Exclusion restriction: Again, the quasi-randomisation ensures that the instrument can only impact the outcome through the take-up of the WYP program.
3. Monotonicity: We can safely rule out that our choice of instrument actually induced some people to be less likely to receive WYP, as the intervention was only offered in 'on' weeks.
4. Non-zero association between instrument and treatment variable: The F-statistic for the first stage regression (e.g. predicting who receives treatment using the instrumental variable) is 705 for our analysis. Using Staiger and Stock's (1997) rule of thumb that the F-stat of the model be greater than 10, this first-stage model is clearly strong enough for this analysis.

The quasi-randomisation to treatment conditions means that we can be near certain that the instrument is unconfounded with respect to whether the individual received the WYP program and uncorrelated with factors influencing the outcome variable (other than through the effect on treatment).

Statistical model

We estimate the impact of the WYP program using the following statistical model.

$$y_i = \beta \cdot \text{treatment}_i + \gamma_j \cdot \text{demographics}_{ij} + \omega_k \cdot \text{prior offending}_{ik}$$

where:

- treatment_i is the treatment indicator;
- β is the estimated impact of the WYP program;
- γ_j is a vector of coefficients for the demographic variables included in the regression;
- ω_k is a vector of coefficients for prior offending.

Definitions of all variables are provided in the variables section preceding this section.

We estimate two different measures of the treatment effect. First, we compare differences in each of the outcome measures between those who were assigned to the WYP program (regardless of whether they agreed to participate) and those who were not. This "intention-to-treat" estimate can be interpreted directly as the estimated average causal impact of the program on individuals when compared with BAU. We use logistic regression for the "any new" ADVO breach/DV offence outcomes and censored regression (tobit) for the days to first ADVO breach/DV offence outcomes.

Second, we estimate the impact of the WYP program on those who agreed to participate in the intervention using an instrumental variables approach (the "treatment-on-treated" estimate).[14] Our "instrument" is whether the individual was allocated (or not) to the WYP program, while the treatment indicator is simply whether they received WYP after being allocated to do so.[15] For the likelihood of the outcome occurring, we estimate an instrumental variables logit model, and for the days until the outcome occurs, we estimate an instrumental variables censored regression (tobit) model.[16] The standard errors

14 For this analysis, we have defined this using the unbiased IV estimator under known first-stage sign proposed by Andrews and Armstrong (2017).
15 This approach gives us a consistent estimate for the causal impact for just those that received WYP, leveraging the quasi-randomisation of the program. If we were to simply drop all individuals who didn't receive WYP and take the average outcome of those that did receive WYP (the 'per protocol' estimate), this estimate would likely be biased by those who select into receiving the WYP having a different likelihood to offend than those who do not choose to receive the program.
16 For each of the instrumental variable estimates, we use the analogous statistical model above for both stages. In the 'first-stage' we use the instrument to predict whether the defendant receives WYP, in addition to all the covariates described above, while in the second stage we use the predicted values from the first stage in addition to all the covariates described above to estimate the treatment-on-treated.

are substantially larger for the instrumental variable estimates, which reflects the additional uncertainty in estimating the effect while accounting for possible self-selection into receiving the treatment. All reported standard errors are heteroskedacity consistent 'robust' errors.

We have restricted our estimates to aggregate effects, rather than present a series of sub-group analyses, because the program was designed to be uniformly delivered across demographic groups and evidence from the process evaluation suggested that WYP was generally implemented as intended across all courts.[17] The analysis presented here has adhered as close as possible to the original pre-specified analysis plan as devised by BOCSAR, NSW BIU and ASU.[18]

Descriptive statistics

Table 1 compares the demographic characteristics and prior criminal history of defendants allocated to a WYP treatment week during the trial period with defendants who attended court in the comparison (BAU) weeks.

Table 1. Characteristics of defendants allocated to the treatment and comparison weeks

Characteristic	On week, N = 1,918[1]	Off week, N = 1,833[1]	*p*-value[2]
Median age	33 (26, 42)	32 (26, 42)	0.018
Unknown	0	1	
Gender			0.500
Female	559 (29%)	514 (28%)	
Male	1,359 (71%)	1,319 (72%)	
Region			0.004
Metro	355 (19%)	297 (16%)	
North/West	342 (18%)	383 (21%)	
Northern	248 (13%)	265 (14%)	
Southern	341 (18%)	267 (15%)	
Western	632 (33%)	620 (34%)	
Unknown	0	1	
Remoteness			0.130
Major cities	442 (34%)	388 (33%)	
Inner regional	449 (35%)	423 (35%)	
Outer regional	257 (20%)	219 (18%)	
Remote	103 (8.0%)	130 (11%)	
Very remote	36 (2.8%)	33 (2.8%)	
Unknown	631	640	
Previous ADVO breach (within five years)	287 (15%)	284 (15%)	0.700
Previous DV offence (within five years)	799 (42%)	737 (40%)	0.400

[1] Median (Interquartile Range) or Count (proportion) are presented in each column as appropriate
[2] Wilcoxon rank sum test; Pearson's Chi-squared test

17 We did estimate treatment effects by ACCSO, as well as perceived differences in enthusiasm for the program, as a robustness check. We did not find any meaningful differences.
18 This pre-specified analysis plan was agreed upon internally, but was not registered publicly.

Overall, the balance check provides evidence for the successful pseudo-randomisation to treatment using the alternating on/off week design. As shown in Table 1, this randomisation strategy resulted in groups that are similar on observables. We find a statistically significant difference for only two variables; the median age and the region where the individual resides. For the median age, the difference is less than one year and we do not believe that this difference is meaningful for this evaluation. For region, there are approximately 3% more participants in the WYP group from the Southern region than in the BAU group (18% vs 15%), and 3% fewer participants are from the North/West region (18% vs 21%). Given these differences are relatively small, we do not believe that they pose any issues for the analysis. Nevertheless, this variable has been included in all analyses to account for any confounding caused by this imbalance.

RESULTS

We estimate the causal impact on breaches of ADVOs and DV related offending, using two measures of each offence:

1. Whether they commit the offence within the observation period;
2. The number of days until the offence occurred (if ever).

For both these outcome measures, we consider three observation periods (three, six and twelve months after the individual is identified by the ACCSO and allocated to either receive WYP or receive BAU) and two estimates of impact (outcome for those allocated to receive WYP and those who actually received WYP). This means that for each offence, we present 12 estimates (2 measures x 3 observation periods x 2 estimates of the outcomes) next to the average outcome in the BAU group. The main results of the analysis are presented below. Full regression tables are provided in the Appendix.

Each of the figures below visualises the level of the outcome (either the proportion or average days) for the business-as-usual cohort, along with level of the outcome implied by our regression model for each of our two estimates (Allocated WYP and Received WYP). This means that the level of each estimate can be straightforwardly interpreted as the predicted outcome for the business-as-usual group had they been allocated to or received WYP respectively.[19]

Breach of Apprehended Domestic Violence Orders

First, we estimate the causal impact of the WYP program on ADVO breaches. As described above, we present the average estimated outcome by treatment status across multiple observation periods. Across all measures, we find no evidence that the WYP program caused a meaningful difference in if or when the individual breached their ADVO.

Whether a breach occurred (measured at three, six and twelve months)

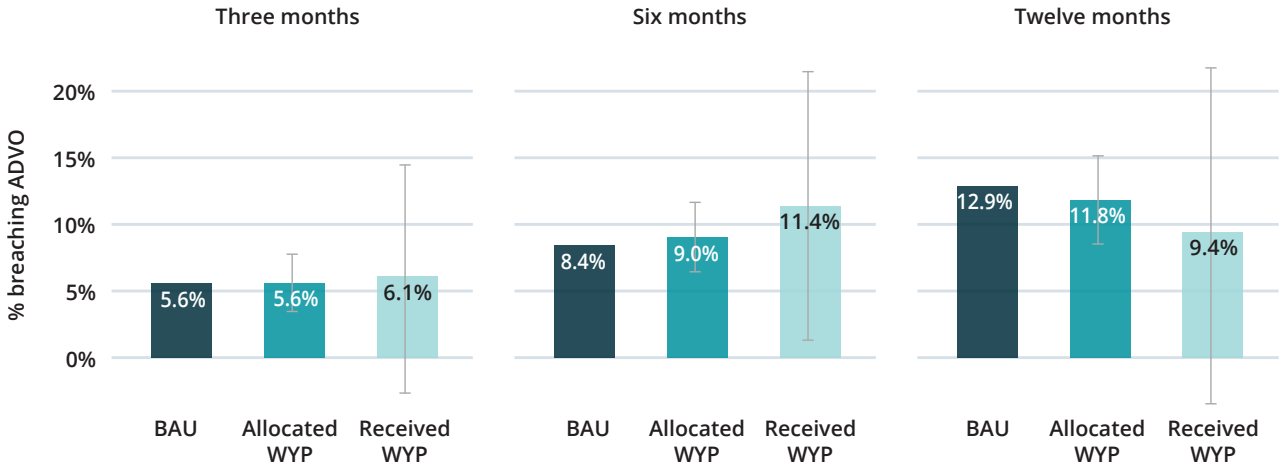
Figure 3 shows only small differences in the proportion of defendants that breached their ADVO across treatment status and observation period. Looking at the three-month observation period, we see that the same proportion of defendants allocated to receive WYP breached their ADVO (5.6%) compared with BAU (5.6%). A slightly *higher* proportion of those that received WYP breached their ADVO (6.1%) compared with BAU, but this difference was not statistically significant at conventional levels.[20]

19 Although this presentation of the regression model is straight-forward to interpret, it can imply values that are not possible in practice. For example, in Figure 4, the model predicts a higher average days than there are days in the observation period, or the confidence intervals for the predictions in Figure 3 predicting negative values. These are largely a consequence of the modelling approach and the BAU group having outcomes very close to the boundaries. As these predictions are implications of our statistical model these have been left unadjusted for transparency.
20 The estimated confidence intervals shown in Figure 3 and 5 contain negative values, something that is not possible with this outcome measure. This is a limitation of our estimation approach, but we believe that the confidence intervals still provide an accurate indication of the precision of our estimate.

Turning to the six-month and twelve-month observation periods, we see larger differences in breach rates by treatment status but the direction of the difference is not consistent across observation periods and no differences are statistically significant at conventional levels. At six months, 0.6 p.p. *more* defendants who were allocated to WYP breached their ADVO compared with defendants allocated to BAU, a difference that increases to 3 p.p. when considering just those who received WYP. However, at twelve months, 1.1 p.p. *fewer* defendants allocated to WYP breached their ADVO compared with defendants allocated to BAU, and 3.5 p.p. *fewer* defendants receiving WYP breached their ADVO.

This analysis provides no evidence that the WYP program had a causal impact on the likelihood of a defendant breaching their ADVO. The direction of the estimates is inconsistent between observation periods, and the small differences observed are within the variation we would expect.

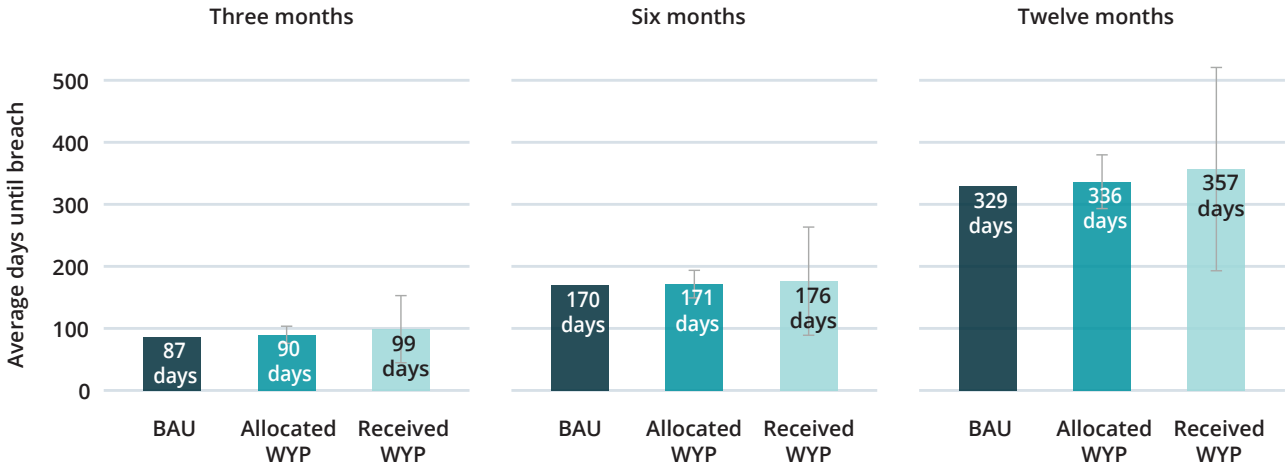
Figure 3. Proportion of defendants that breached their ADVO, by treatment status and observation period



Days until a breach occurred, if ever (measured at three, six and twelve months)

Figure 4 shows the average number of days until first ADVO breach occurred (if ever), by treatment status and observation period. This measure allows us to understand whether WYP has been effective in delaying breaches. The results displayed in Figure 3 are consistent with the results shown above.

Figure 4. Average days until the defendant breached their ADVO (if ever), by treatment status and observation period



Looking at the three and six-month observation periods, we see small differences by treatment status in the average number of days until an ADVO breach occurs. At three months, there is a difference of 3 and 12 days, respectively, for those allocated and those that received WYP compared with those in the BAU condition. At six months, there is a difference of 1 and 6 days, respectively, for those allocated and those that received WYP compared with the BAU condition. These differences are too small for us to have any certainty that they can be attributed to the WYP program.

At the twelve-month mark, the difference between the groups is much larger, both proportionally and in absolute magnitude. For those who received WYP, breaches occurred on average 28 days later than for those in the BAU condition. However, there are wide confidence intervals associated with this IV point estimate of the treatment effect (the treatment-on-treated estimate) and the difference is not statistically significant at conventional thresholds.

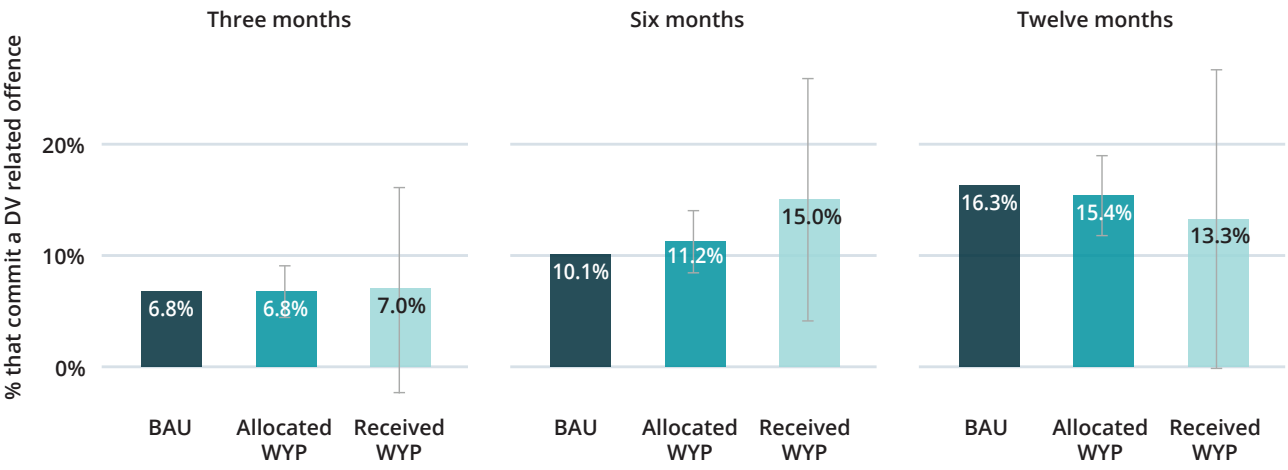
Domestic Violence related offending

Next, we estimate the causal impact of the WYP program on the likelihood of any DV related offence. Analogous to the analysis of ADVO breaches, we measured this outcome in two ways: (1) the probability that a DV related offence occurred at any time during the observation period, and; (2) the number of days until a DV related offence occurred (if ever).

Whether a DV related offence occurred (measured at three, six and twelve months)

Figure 5 shows the proportion of defendants who committed a DV related offence, by treatment status and observation period. We see a similar pattern of results across the observation periods as was found for ADVO breaches. The proportion of defendants who committed a DV related offence within three months of entering the trial was the same for those allocated to WYP and those allocated to BAU, with 6.8% of defendants in both groups committing a DV related offence during this period.

Figure 5. Proportion of defendants that committed a DV related offence, by treatment status and observation period



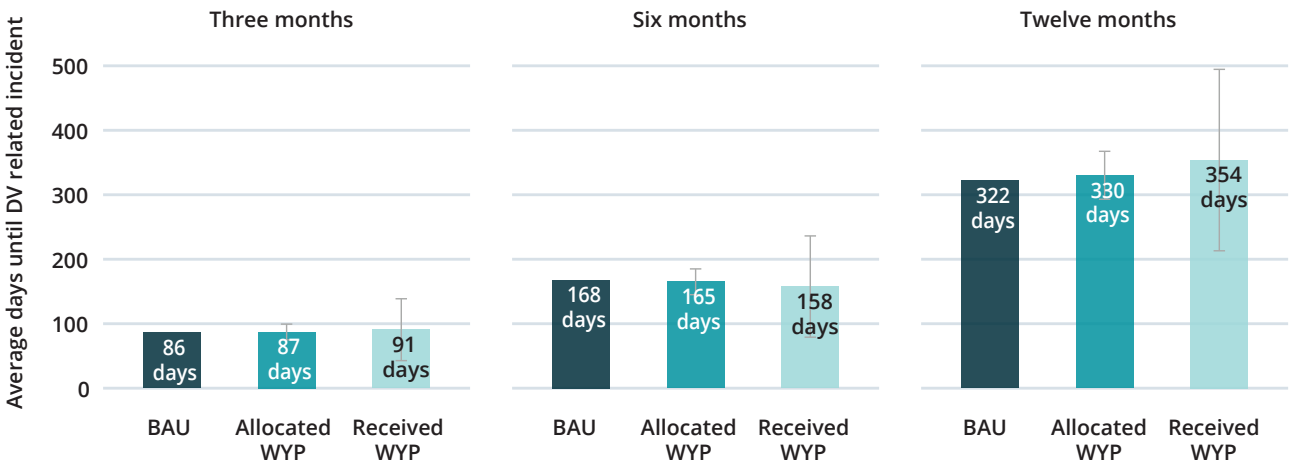
At the six-month observation period, we again observe *higher* rates of DV related offending for those allocated to and those that received WYP (1.1 p.p. and 4.9 p.p. respectively) when compared to the BAU condition, but these differences are not statistically significant. Over the twelve-month observation period, we see that both the allocated and received WYP groups had a lower proportion of defendants who committed a DV related offence (0.9 p.p. and 2.1 p.p. respectively) compared with BAU, but again these differences are not statistically significant.

This analysis provides no evidence that the WYP program had a causal impact on the likelihood of a defendant committing a DV related offence. The small differences between groups and the inconsistent direction of the treatment effect suggests that the WYP program was not sufficient to cause a meaningful change in DV related offending.

Days until a DV related offence occurred, if ever (measured at three, six and twelve months)

Figure 6 shows the average number of days until a DV related offence occurred (if any), by treatment status and observation period. Consistent with the analysis of days until any ADVO breach, we see very small differences between defendants receiving BAU and those that were allocated to or received WYP at the three-month mark (1 day and 5 days, respectively) and the six-month mark (3 days and 10 days respectively); and none of these differences are statistically significant. The differences between these groups are substantially larger when measured at 12 months follow-up (8 days for those allocated to WYP and 32 days for those that received WYP) but are not statistically significant at conventional levels.

Figure 6. Average number of days until the defendant committed a DV related offence (if ever), by treatment status and observation period



This analysis does not provide any evidence that the WYP program had a causal impact on DV related offending behaviour. In line with the previous results, the differences between groups are small and we cannot confidently attribute them to the WYP program.

DISCUSSION

This study estimates the causal impact of the WYP program on breaches and DV related offending amongst Aboriginal defendants issued an ADVO in NSW. Defendants were quasi-randomised to the brief intervention using an "on week/off week" research design. Outcomes for those who were allocated to receive WYP were compared with outcomes for those that were not allocated and did not receive WYP. ADVO breaches and proven DV offences were measured at three, six and twelve months after delivery of WYP. Comparing both the likelihood and time to first new offence/breach, we found no meaningful differences between defendants allocated to receive the program and defendants allocated to the BAU condition for any of the outcomes measured. Restricting the analysis to those who were offered and accepted the WYP program similarly found little evidence for an effect of WYP on ADVO breaches or DV offences.

The aim of the WYP program was to improve compliance with ADVOs through the use of mental contrasting and implementation intention techniques to help close the gap between a defendant's intention to comply with their ADVO and their actions. There are four possible explanations for the small differences observed between those that received the intervention and those that did not:

1. The intervention was not successful in closing the 'intention-action' gap;
2. Closing the 'intention-action' gap was not sufficient to change breach or domestic violence behaviour;
3. Recorded breach and domestic violence offences are not a sufficiently sensitive measure to detect the change in behaviour;
4. Failure of implementation and/or randomisation.

From the data we have available it is difficult to assess the relative importance of each of these explanations for the results reported here. The study provides evidence that the delivery of WYP was not sufficient to induce large changes in the criminal justice outcomes examined, but this does not necessarily imply that the MCII intervention was not successful in closing the intention-action gap (i.e. the intended mechanism of action). Additional self-reported data on compliance with the if-then plans developed by the defendants during the trial would be needed to assess whether this shorter-term goal was achieved.

In any case, even if WYP was successful in closing the intention-action gap, it is possible that the brief 10-minute intervention was not sufficient for most individuals to avoid breaching their ADVO or committing further DV related offences. The drivers of domestic violence are complex, with financial stress, personal stress, substance abuse, lack of social support and trauma being just a few of the interconnected factors that drive offending behaviour (DeMaris, Benson, Fox, Hill & Van Wyk, 2003; Lauritsen & Schaum, 2004; Yakubovich et al., 2018). There is weak evidence for the effectiveness of *any* perpetrator interventions for domestic violence (Akoensi, 2013; Eckhart et al., 2013), suggesting that this is a particularly difficult behaviour to shift. Brief interventions, such as WYP, may need to be combined with more intensive behavioural change programs and/or long-term social support to have any impact on rates of DV offending.

Another limitation of our study is its reliance on administrative data to assess program effectiveness. It is well established that DV is significantly undercounted in criminal justice administrative data. Only about one-half (51.8%) of all DV offences are reported to police and many of these do not proceed to the conviction stage (the threshold for inclusion here) (Birdsey & Snowball, 2013). It is possible that the WYP program resulted in small changes in perpetrator behaviour and victim safety but the outcome measure used was not sensitive enough to detect these incremental improvements.[21] Inclusion of self-report data

21 For this study to distinguish between these explanations, we would need more proximate measures such as the relationship with the ACCSOs, actions taken toward avoiding a breach and/or re-offending and a closer understanding of if (and how) the WYP program altered behaviour for those who received the program. This would have considerably expanded the scope and cost of the research, as these measures would need to have been collected for the project and added considerable burden on the implementation of the study.

from victims and defendants would have significantly enhanced the current evaluation. Self-report data would not only have improved the accuracy of our outcome measures but could also have been used to explore defendants' satisfaction with the intervention, whether WYP changed their interaction with the ACCSOs or improved their broader experience with the justice system, and the extent to WYP helped them to reframe ADVO compliance.

The validity of our estimates also depends on the quasi-randomisation implemented by the ACCSOs for the WYP evaluation. For example, it is possible that there were "spill-over effects", through ACCSOs becoming upskilled and/or compensating in "off weeks" to deliver equivalent support, which would lead us to understate the benefit of the WYP approach.[22] While there were a small number of issues identified in the process evaluation (e.g. few ACCSOs had experience delivering a program like WYP, lengthy delays between initial training and completing WYP sessions), the implementation of WYP was largely considered to be a success (Nelson, 2018). This is especially impressive given the burden the evaluation design placed on the ASU workforce, and the diversity and varying levels of experience and expertise of the ACCSOs (Nelson, 2018).

The successful implementation of WYP, and the NSW Department of Communities and Justice Aboriginal Services Unit's commitment to rigorous evaluation, has also generated a wealth of valuable experience, data and materials for both ACCSOs and policy makers. For example, the plans that have been collected as part of the WYP program could be analysed and used to compare the anticipated causes of breaches with the eventual reasons for a breach occurring (if any). Further research with defendants who have gone on to commit a breach would assist in identifying how interventions and resources could be better targeted to prevent a breach from occurring in similar scenarios.

Given the successful implementation of WYP and the enhanced capabilities of the ASU workforce evidenced by the implementation of the trial, consideration could be given to trialling a more intensive ACCSO delivered intervention or a set of interventions to assess whether enhancements to the current model would be sufficient to shift outcomes. Any future reforms should continue to incorporate two critical elements that were successful elements of the 'What's Your Plan' program: (1) an extensive development and co-design process with Aboriginal staff and; (2) the establishment of a robust evaluation methodology ahead of program implementation to test whether stated objectives are achieved.

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22 One way to test whether ACCSOs have become up-skilled over time in preventing breaches and/or domestic violence related offending from occurring is to look at whether these outcomes have shifted over time for defendants at these courts. We observe no meaningful differences in outcomes both across the duration of the trial or after the trial has ended.

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