

CRIME AND JUSTICE BULLETIN

NUMBER 241 | SEPTEMBER 2021

The effect of appointing additional judges on District Court finalisations

Steve Yeong and Sara Rahman

AIM To estimate the effect of appointing seven new District Court (DC) judges on the monthly count of finalisations in the DC.

METHOD We use an extract from the NSW Bureau of Crime Statistics and Research's Reoffending Database (ROD). The ROD extract contains the monthly count of finalised matters in each DC in NSW over the period 1 January 2014 to 30 September 2020. Using these data, we compare the monthly count of finalisations in a treatment group consisting of courthouses that gained a minimum of 10 additional sitting weeks after the seven new judges were appointed, to a control group consisting of courts that gained exactly zero additional sitting weeks, in a difference-in-differences setup. Courthouses in the treatment group are the Sydney, Newcastle, Wollongong, Gosford, Lismore, and Coffs Harbour DCs. Courthouses in the control group are the Campbelltown, Parramatta, Penrith, Dubbo, Parkes, Coonamble, Bourke, Broken Hill, and Wagga Wagga DCs.

RESULTS The effect of the additional judges varies between courthouses. The Newcastle and Wollongong DCs experienced an additional 3.34 and 5.26 finalisations per month after the appointment of the additional judges. The reforms are not associated with an increase in finalisations in any other courthouse examined. We found evidence to indicate that this may be caused by the new judges being used to alleviate the court's reliance on acting (or temporary) judges and an increase in sexual assault and related trials in some locations. We also cannot rule out that our results are driven by varying trends in finalisations between treatment and control courts over time.

CONCLUSION The DC7 reforms have generated a small increase in the monthly count of finalisations in some courthouses. The reforms have also reduced the court's reliance on acting judges to some extent.

KEYWORDS

District Court

judges

backlog

INTRODUCTION

In New South Wales (NSW), the number of registered trials that are yet to be finalised in the NSW District Criminal Court (DC) has been steadily increasing since 2012 (Thorburn & Weatherburn, 2018). Congestion in the DC is problematic for many reasons, one of which is its impact on the remand population. Given the serious nature of matters finalised at the DC level, many defendants are held on remand prior to the finalisation of their case. In 2012, inmates on remand accounted for 25 per cent of the prison population, but by 2018 this had risen to 33 per cent (New South Wales Auditor-General, 2019). In addition to the adverse economic and social consequences for the individual, a large remand population also generates a significant financial burden for the state. For instance, in the 2017-18 financial year, the annual average cost associated with housing a single inmate was \$66,375 (New South Wales Auditor-General, 2019).

To address this problem, in recent years, a variety of measures have been introduced aimed at reducing the DC backlog. The purpose of this bulletin is to evaluate the effectiveness of one such measure: the appointment of seven additional DC judges (hereafter referred to as 'the DC7 reforms'). However, before describing the DC7 reforms in detail, an overview of other recent measures to address the backlog in the DC is provided.

Prior research

In recent years, a variety of measures aimed at addressing congestion in the NSW DC have been introduced. In this section, we will review five such measures that have been the subject of evaluation.

The first of these, the Rolling List Court, was introduced on 17 March 2015 at the Sydney Downing Centre DC through a collaboration between Crown Prosecutors, Legal Aid, Public Defenders and the District Court. The idea behind the Rolling List Court was to reduce the DC backlog through early resolution of indictable matters. The Rolling List Court involved a dedicated judge and two teams of prosecutors and defence lawyers. While one was at trial, the other team prepared for trials and engaged in negotiations. It was expected that the close relationship and early negotiations between senior legal practitioners would contribute towards earlier resolution of indictable matters. The first year of the Rolling List Court's operation was set up as a randomised controlled trial where eligible cases committed to the DC were randomly allocated to either the Rolling List Court or the regular DC. Rahman, Poynton, and Weatherburn (2017) found that cases assigned to the Rolling List Court were significantly more likely to result in an early guilty plea and took significantly less time to finalise than cases assigned to the control group (i.e., dealt with through the usual court process).

The next set of reforms came about over the 2016-17 calendar years. The 2016-17 reforms had four components: the appointment of five additional DC judges; two additional public defenders; the introduction of readiness hearings¹; and finally, the DC increased special call-overs.² To evaluate these reforms, Thorburn and Weatherburn (2018) examined the monthly count of DC finalisations before and after the reforms. Although ultimately Thorburn and Weatherburn (2018) were interested in the effect of these measures on the DC backlog, they chose to examine the effect of these measures on the monthly rate of finalisations. This is because the size of the backlog is influenced by both: the number of new cases (i.e., the 'inflow'); and the number of finalised cases (i.e., the 'outflow'). As such, direct examination of the backlog could yield misleading results if, for example, an increase in the outflow (generated through one of the measures examined by Thorburn & Weatherburn, 2018) was offset by an increase in the inflow of cases (generated through improvements in policing). Thorburn and Weatherburn (2018) found that each additional judge was associated with a monthly increase of 8.5 additional finalisations. They also found that the special call-overs generated sizable increases in the number of DC finalisations. Thorburn and Weatherburn (2018) did not, however, find that the readiness hearings or additional public defenders generated any (detectable) effect on the rate of DC finalisations (although, in practice, the special call-overs would not have been possible absent the appointment of additional public defenders).

¹ Readiness hearings are preparatory meetings designed to ensure that the prosecution and defence are able to begin the trial on the scheduled start date.

² Special call-overs are designed to generate a one-off increase in District Court finalisations. This is achieved by offering defendants a discount on the prison time associated with their case in exchange for a guilty plea.

The Table Offences Reforms were the next set of measures introduced in NSW to address the DC backlog. The Table Offences Reforms involved a series of legislative changes that allowed specific offences, previously required to be finalised in the DC, to be finalised at the Local Court (LC) level. The Table Offences Reforms were implemented in two phases. Phase 1 was introduced in 2016 and applies to a small collection of break and enter offences. Phase 2 was introduced in 2018 and applies to a collection of theft, justice procedure offences, robbery and illicit drug offences. Ringland (2020) examined the first phase of the Table Reforms by comparing cases occurring in the two years before and after the reforms. She found that the number of finalisations for reform-related charges increased by 26 per cent after implementation of the reforms. Overall, 85% of eligible offences were diverted from the DC, resulting in a 6-month reduction in time from charge to finalisation. Ringland (2021) found very similar effect sizes in a later evaluation of the second phase of the Table Offences Reforms.

Around the same time as Phase 2 of the Table Offences Reforms commenced, the Early Appropriate Guilty Plea (EAGP) reforms were introduced for charges commencing on or after 30 April 2018. The EAGP reforms centred around two themes. The first involved streamlining the committal process and the second involved changes to the incentive structure faced by prosecutors and defendants. Streamlining the committal process involved: a) simplifying the way in which police provide evidence to the court; b) mandating senior defence and prosecution lawyers to participate in early discussions about the case; and c) shifting the onus from LC magistrates to the Office of the Director of Public Prosecutions to determine which cases are committed to the DC; d) changing the incentive structure focussed on limiting the capacity for plea negotiations to occur at relatively late stages in proceedings by requiring senior prosecutors to certify (or 'lock-in' charges) prior to committal; and e) by introducing statutory sentencing discounts.³ Klauzner and Yeong (2021) found that the EAGP reforms were associated with significant increases in the likelihood of an early guilty plea among matters committed to the DC, and a small increase in finalisations per month.

The primary conclusion to be drawn from these studies is that, by-and-large, the recent measures introduced in NSW have been successful in increasing efficiency in the DC. Unfortunately, however, these attempts to increase the 'outflow' of cases have been met by an increase in the 'inflow' of cases (NSW Bureau of Crime Statistics and Research, 2020). As such, reducing congestion in the DC remains an ongoing concern for government. The contribution of this bulletin is to examine another measure introduced to further reduce the DC backlog: the DC7 reforms.

The DC7 reforms

Recall that Thorburn and Weatherburn (2018) found the appointment of five additional DC judges generated a sizeable increase in the monthly number of state-wide DC finalisations. This success, at least in part, resulted in the government appointing seven new DC judges in February 2019 (NSW Government, 2018). During their first year, the judges were based primarily at the Sydney Downing Centre.⁴ Two of the judges were allocated to deal with the expected increased volume of civil matters resulting from the Royal Commission into Institutional Responses to Child Sexual Abuse. The remaining five were allocated to deal with criminal matters in the DC. The Sydney Downing Centre has been close to capacity for several years. As such, when the new judges began presiding over cases in Sydney, some of the incumbent judges began presiding over cases in regional courts. Thus, in 2019, there was a (net) increase in the number of judges operating in both the Sydney Downing Centre and a handful of regional courts.

The mechanism through which these judges may have increased the monthly finalisation rate is additional sitting weeks (i.e., time that judges spend presiding over cases). Prior to the announcement of the DC7 reforms on 29 October 2018, each courthouse's sitting weeks had already been scheduled for the 2019

³ That is, a defendant can receive a 25% discount on their sentence if they enter a guilty plea while the matter is at the LC, 10% after the matter moves to a higher court, and 5% on the day of the trial.

⁴ There were two reasons why judges worked at the Sydney Downing Centre instead of immediately moving to regional DCs. First, to receive judicial education, which involves a variety of conferences and seminars designed to keep judges up to date with the latest developments in the law, court procedure and community values (Judicial Commission, 2020). Second, to establish relationships with incumbent judges. These relationships are important as they enable a newly appointed judge to learn from their peers and seek advice when necessary.

calendar year.⁵ This information is provided in the first column of Table 1. The actual number of sitting weeks in each courthouse is provided in the second column of Table 1. The difference between columns 1 and 2 (shown in column 3) indicates how many additional sitting weeks each courthouse experienced over the 2019 calendar year.

From Table 1 we can see that some courthouses gained many sitting weeks (e.g., the Newcastle DC gained an additional 33), some courthouses gained only a handful (e.g., the Campbelltown DC gained four) and other courthouses gained none (e.g., Parramatta). We are, unfortunately, unable to precisely determine why some courthouses gained more additional sitting weeks than others. For example, the Port Macquarie DC received one additional sitting week. One explanation is that this is the result of increased capacity at the Port Macquarie DC generated through the reforms. Another explanation is that one of the judges primarily working out of the Port Macquarie DC took less leave than had been planned in 2018. Alternatively, it could also be the case that the Port Macquarie DC experienced a higher than expected caseload such that an additional incumbent (permanent) or acting judge was assigned to the courthouse.

Given the uncertainty around how and why some courthouses experienced more sitting weeks than others, we take a conservative approach by assigning courthouses that gained at least 10 additional sitting weeks to the treatment group (i.e., courthouses affected by the DC7 reforms), courthouses that gained zero sitting weeks to the control group (i.e., courthouses unaffected by the DC7 reforms) and exclude courthouses that gained between one and nine sitting weeks from the analysis entirely.⁶

Before moving on to describing the data, another point worth mentioning is that the sum of additional sitting weeks is 478. Given that each DC judge is expected to generate 40.6 sitting weeks per year, this implies a state-wide increase of about 12 judges, not five (as two of the seven were assigned to address civil matters which we do not observe). A potential reason for this inconsistency is the appointment of six full time equivalent acting judges to the Sydney DC (Garvey, 2020). In fact, if the Sydney DC is excluded from the calculation, there is only an additional 155 sitting weeks (the full time equivalent of 3.86 judges).

⁵ Sitting weeks refer to the aggregate time (in weeks) that judges preside over cases within a given courthouse. A single judge working full time is expected to generate approximately 40.6 sitting weeks per year.

⁶ Acknowledging that this choice is arbitrary, we test the robustness of our results when courts who experienced a 25% or greater increase in sitting weeks are included in the treatment group in the Appendix. We find that our overall estimate does not differ when using this alternative rule to identify treatment courts.

Table 1. Scheduled vs. actual sitting weeks over the 2019 calendar year

Courthouse	(1) Scheduled	(2) Actual	(3) Additional	(4) Group
Sydney	810	1,133	323	Treatment
Parramatta	349	349	0	Control
Penrith	90	90	0	Control
Campbelltown	133	137	4	N/A
Newcastle	148	181	33	Treatment
Port Macquarie	20	21	1	N/A
Taree	18	19	1	N/A
Tamworth	23	30	7	N/A
Moree	6	9	3	N/A
Gosford	63	82	19	Treatment
Wollongong	61	80	19	Treatment
Nowra	19	23	4	N/A
Queanbeyan	14	15	1	N/A
Goulburn	18	23	5	N/A
Bega	14	17	3	N/A
Lismore	52	73	21	Treatment
Coffs Harbour	32	46	14	Treatment
Armidale	20	23	3	N/A
Grafton	11	12	1	N/A
Dubbo	43	43	0	Control
Orange	13	17	4	N/A
Bathurst	14	16	2	N/A
Parkes	4	4	0	Control
Coonamble	2	2	0	Control
Bourke	3	3	0	Control
Broken Hill	15	15	0	Control
Wagga Wagga	49	49	0	Control
Albury	18	23	5	N/A
Griffith	24	29	5	N/A
Total	2,086	2,564	478	

Note: N/A = excluded from the estimation sample.

METHOD

Data

We utilise an extract from the NSW Bureau of Crime Statistics and Research's Reoffending Database (ROD) for this study. The ROD extract contains information for every criminal matter finalised in an NSW DC between 1 January 2014 and 30 September 2020.⁷ For each finalised matter, which we refer to as a 'case', we are able to observe: information pertaining to the defendant (e.g., date of birth, sex, Aboriginality⁸, number of prior court appearances and sentences of imprisonment), information pertaining to the case (e.g., the number and nature of each charge⁹) the date of the finalisation; a unique (de-identified) numerical code for each judge; a flag for whether the judge was permanent or acting; and finally, the location of the courthouse where the case was finalised.

Descriptive statistics

In order to examine the effect of the DC7 reforms, we reorganise the data into a monthly panel at the courthouse level (i.e., each row constitutes a court-month-year combination).¹⁰ Using this aggregation, we then count the number of cases finalised within each courthouse over the sample period.¹¹

Table 2. Average monthly number of finalisations by courthouse and group allocation

	Pre-policy		Post-policy		Difference	
	Mean	Std. Dev.	Mean	Std. Dev.	Estimate	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Treatment group						
Coffs Harbour	5.177	4.123	4.421	2.735	-0.756	(0.812)
Gosford	12.823	7.469	13.526	5.327	0.704	(1.536)
Lismore	11.048	6.323	9.158	5.984	-1.890	(1.575)
Newcastle	24.613	11.477	30.368	11.369	5.756	(2.958)
Sydney Downing Centre	123.242	32.566	122.684	32.929	-0.558	(8.526)
Wollongong	10.613	7.21	14.842	5.408	4.229**	(1.530)
Panel B. Control group						
Bourke	0.565	1.018	0.368	0.761	-0.196	(0.216)
Broken Hill	1.694	2.584	1.158	1.302	-0.536	(0.442)
Coonamble	0.484	0.987	0.263	0.562	-0.221	(0.179)
Dubbo	7.274	4.312	5.105	3.195	-2.169*	(0.908)
Parkes	0.903	1.141	1.053	1.268	0.149	(0.322)
Parramatta	47.048	17.388	53.632	16.647	6.583	(4.369)
Penrith	16.855	11.228	19.263	8.171	2.408	(2.338)
Wagga Wagga	8.323	4.935	6.632	3.253	-1.691	(0.968)

Note. Robust standard errors in parentheses, * $p < .05$ ** $p < .001$ *** $p < .001$

⁷ Note that the follow-up period overlaps with significant disruptions to the courts because of the COVID-19 pandemic from March 2020 onwards. However, the method we use is robust to these changes as they occur for both the treatment and the control group, and our conclusions remain qualitatively similar when the analysis is restricted to matters finalised before 28 February 2020. The results of these additional sensitivity analyses are available on request.

⁸ Aboriginality can be measured two ways: the first is whether the person identified (to police) as Aboriginal when charged; and second, whether the person has ever identified as Aboriginal to police. We use the former of these measures.

⁹ The nature of each charge is determined using the Australian and New Zealand Offence Classification (ANZSOC) codes. Interested readers are directed to ABS (2011) for further information regarding ANZSOC codes.

¹⁰ In Table A1 of the Appendix we check the robustness of the results to a quarterly level of aggregation.

¹¹ Following Thorburn and Weatherburn (2018), we examine the monthly rate of finalised cases instead of the backlog to avoid the problem of an increase in the inflow of cases masking the effect of an increase in the outflow of cases. This is described in further detail in both the literature review section of this bulletin and by Thorburn and Weatherburn (2018).

Table 2 reports the average monthly count and standard deviation of finalisations in each of the DCs assigned to either the treatment or control groups. From Table 2 we can see a statistically significant increase in the Wollongong DC. There also appears to be an increase in the Newcastle and Gosford DCs, although the difference is not statistically significant at the five per cent level. Interestingly, the Sydney, Lismore and Coffs Harbour DCs do not appear to have experienced an increase. If anything, these courthouses appear to be experiencing fewer finalisations each month. This may, however, be simply a reflection of a downward state-wide trend in finalisations. For instance, from Panel B we can see that the majority of the control courts also experienced a (statistically insignificant) decrease in finalisations after the introduction of the reforms.

Another salient feature of Table 2 is the Sydney DC. The Sydney DC appears to finalise five times as many cases as the next largest treatment court and almost triple as many cases as the largest control. This makes the Sydney DC a significant outlier. The next section elaborates on how we address this issue.

Empirical approach

In order to identify the causal effect of the DC7 reforms on the monthly count of finalisations, we employ a difference-in-differences approach. The intuition is to compare a treatment group of courthouses affected by the reforms to a control group of courthouses unaffected by the reforms, before and after the introduction of the reforms.

Recall from Table 1 that we assign courthouses that gained at least 10 additional sitting weeks to the treatment group, courthouses that gained zero sitting weeks to the control group and exclude courthouses that gained between one and nine sitting weeks from the analysis entirely. Therefore, our treatment group consists of the Sydney, Newcastle, Gosford, Wollongong, Lismore, and Coffs Harbour DCs, while our control group consists of the Parramatta, Penrith, Dubbo, Parkes, Coonamble, Bourke, Broken Hill, and Wagga Wagga DCs.

Using these treatment-control allocations, we then estimate a Negative Binomial regression of Equation 1.¹²

$$y_{it} = \beta D_{it} + \theta_i + \lambda_t + \varepsilon_{it} \quad (1)$$

In Equation 1, y_{it} denotes the count of finalisations in courthouse i during month-year t . D_{it} is a binary variable equal to one for courthouses in the treatment group after the DC7 reforms, zero otherwise. θ_i denotes a set of courthouse fixed effects. These fixed effects render the estimates robust to systematic differences between courthouses that have not changed between January 2014 and September 2020 (e.g., the fact that the Sydney DC finalises such a disproportionately large share of cases). λ_t denotes a set of month-by-year fixed effects. These fixed effects render our estimates robust to factors common to all courthouses that change over time (e.g., legislative changes like the Table and EAGP reforms, the NSW unemployment rate, seasonality in crime etc). ε_{it} is the error term.

The coefficient of interest, β , represents the change in each courthouse's monthly count of finalisations, among courthouses in the treatment group, associated with the DC7 reforms (i.e., the average treatment effect on the treated).¹³ In order for β to be interpreted as the causal effect of the DC7 reforms, the control group must be able to provide a conditionally valid counterfactual for the treatment group (i.e., tell us what would have occurred to the treatment group in the absence of the DC7 reforms).

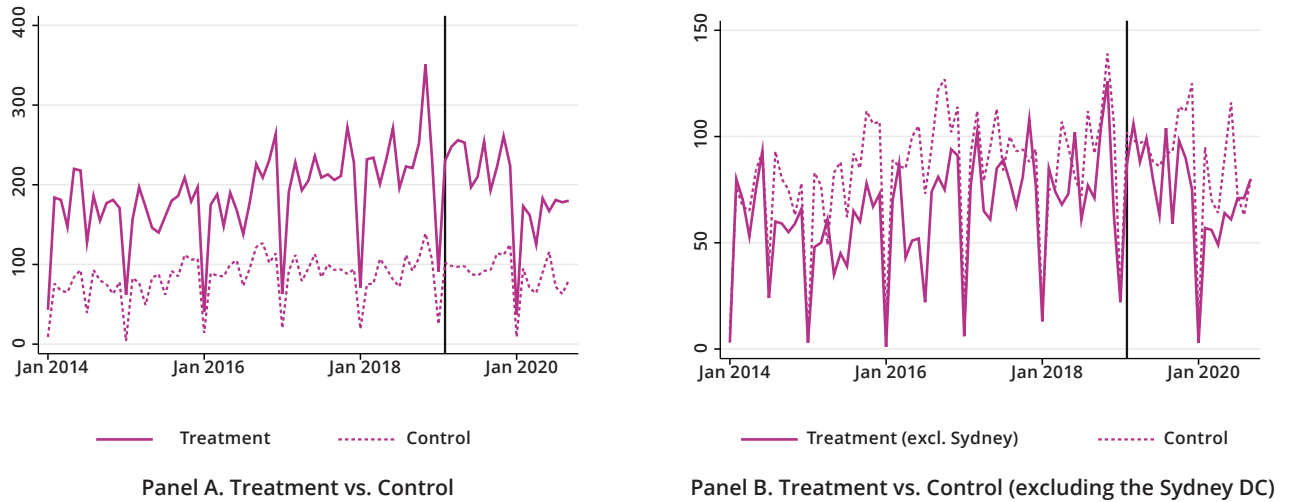
While there is no way to formally test this assumption, the most common technique for determining its validity is to examine the pre-policy trends of both groups. If the control group provides a (conditionally) valid counterfactual outcome for the treatment group, we would expect the two groups to share common pre-policy trends. That is, while the treatment and control courts can differ in *levels* (e.g., one group may finalise a larger *volume* of cases), the two groups' outcomes must move in the same direction (i.e., the *trend* in both groups must be either stable, trending up, or trending down, so long as they are moving together).

¹² We test the robustness of the findings to a linear regression specification in the Appendix.

¹³ We report average marginal effects for ease of interpretability and robust standard errors clustered at the courthouse level.

Figure 1 provides this graphical analysis. Figure 1 plots the aggregate number of finalisations in the treatment and control courts over the period January 2014 – September 2020. The introduction of the DC7 reforms, in February 2019, is given by the vertical line. Panel A compares the count of finalisations between the treatment and control courts over time. Panel B provides this same comparison but excludes the Sydney DC from the treatment group.

Figure 1. Average monthly count of District Court finalisations: January 2014 – September 2020



From Panel A we can see that the treatment group finalises more cases each month than the control group. However, from Panel B, we can see that this is almost entirely due to the Sydney DC. Once we exclude the Sydney DC from the treatment group, the control group finalises more cases each month. This also has implications for the validity of the common trends assumption. While there is an upward trend in pre-policy finalisations for both groups, in both panels, the upward trend in the treatment group in Panel A appears to be larger than the upward trend in the control group. However, once we exclude the Sydney DC from the treatment group, in Panel B, the two groups appear to follow similar trends. In order to address the fact that the Sydney DC is clearly an outlier, we: a) include a set of court fixed effects when averaging across all courts within a single model; b) examine each treatment court in isolation; and c) include a set of court-by-month-year linear trends in the robustness checks.¹⁴

We also present a formal test of prior trends between courts by estimating a flexible event-study specification. This includes all the leads and lags before and after the introduction of the DC7, excluding the lag before the introduction of the DC7, for our main comparison and the control courts. Specifically, we estimate:

$$y_{it} = \sum_{j=-2}^J \beta_j (\text{Lag } j)_{it} + \sum_{k=0}^K \gamma_k (\text{Lead } k)_{it} + \theta_i + \lambda_t + \varepsilon_{it} \quad (2)$$

Where y_{it} is the number of monthly finalisations, $\sum_{j=-2}^J \beta_j (\text{Lag } j)_{it}$ is the sum of a set of dummy variables for each lag (i.e. month preceding the appointment of the DC7) excluding the last month multiplied by their coefficients, $\sum_{k=0}^K \gamma_k (\text{Lead } k)_{it}$ is the sum of a set of dummy variables for each lead multiplied by their respective coefficients (i.e. months following the appointment of the DC7), θ_i is a set of court fixed effects and λ_t is a set of month fixed effects, and ε_{it} are standard errors clustered at the court level.

¹⁴ These robustness checks are reported in Tables A2 and A3 of the Appendix. Further detail regarding these robustness checks is provided in the next section of the bulletin.

Figure 2. Event study estimates of the impact of DC7 on monthly finalisations in treated courthouses

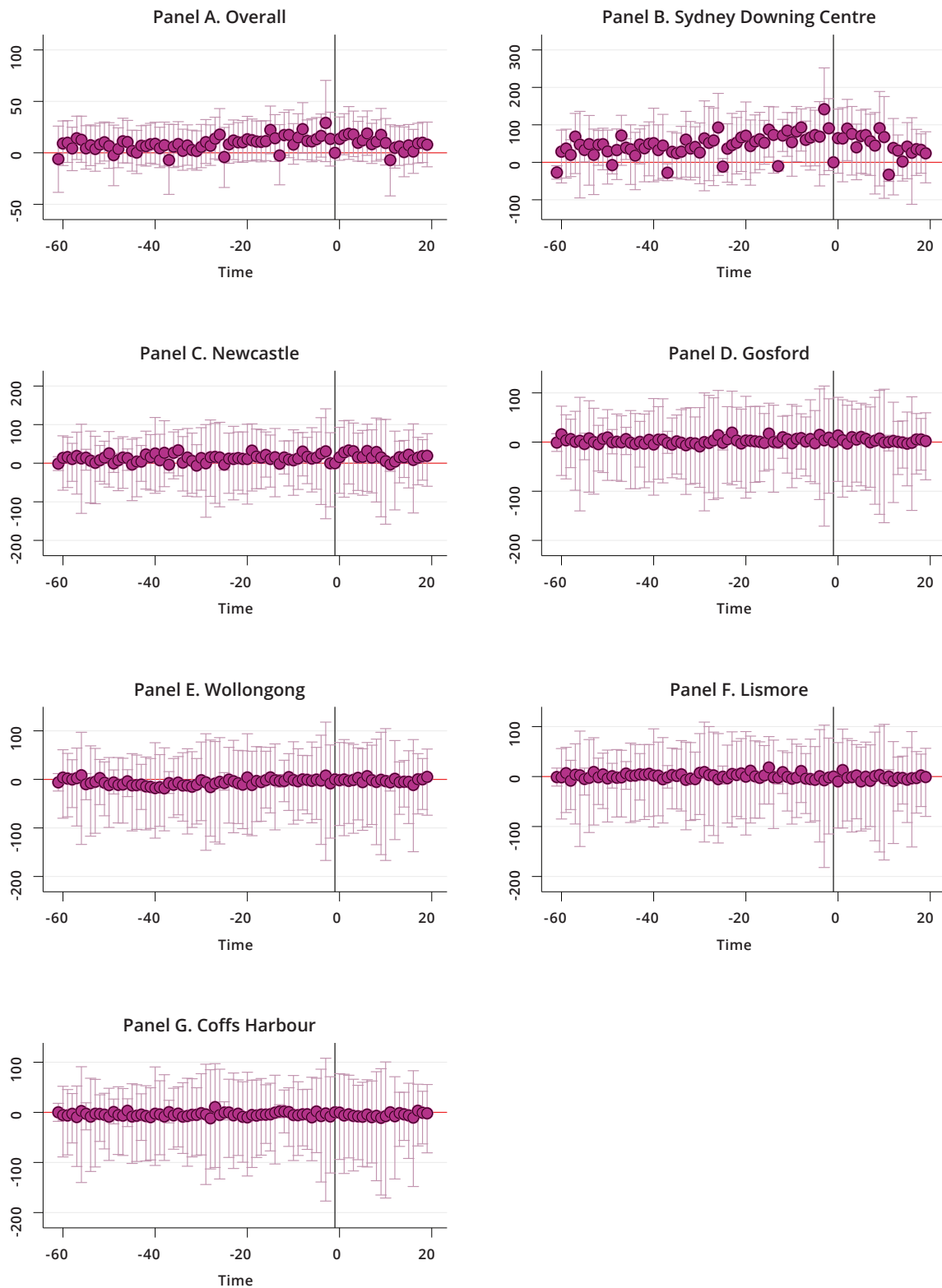


Figure 2 presents a graphical representation of these estimates (we report them in tabular form in the Appendix Table A4). The intuition behind this test is that if the trends between the treatment and control courts are similar before the introduction of the DC7, all the coefficients on the lags of the treatment courts should be zero, (i.e. there should be no differences between the groups' trends in finalisations prior to the introduction of DC7). Examining Figure 2, it appears that our overall comparison is reasonable, although the coefficients appear to get larger closer to the introduction of DC7. A joint F-test of these lags

results in a p-value of .005. While this is statistically significant, overall the coefficients are relatively small and close to zero. Examining the rest of the panels, our comparisons for Wollongong, Lismore and Coffs Harbour appear more credible than those for Sydney, Newcastle and Gosford. Regardless, virtually all our joint F-tests fail to reject that all the coefficients are zero. This means that our difference-in-differences estimates should be considered with caution.

RESULTS

The effect of the DC7 reforms on the monthly count of finalised cases

Table 3 reports the results of our main analysis. Column 1 reports the average marginal effect associated with a negative binomial regression of Equation 1 over the entire sample (i.e., all treatment and control courts defined in Table 1). Columns 2 – 7 report estimates where only a single treatment court is retained and compared to all the control courts.¹⁵

Table 3. Effect of the DC7 reforms on the monthly count of finalisations

	(1) All courts	(2) Sydney	(3) Newcastle	(4) Gosford	(5) Wollongong	(6) Lismore	(7) Coffs Harbour
Estimate	1.931	-0.331	3.336**	1.260	5.262***	-1.539	-0.864
Std. Err.	(2.742)	(2.343)	(1.561)	(1.334)	(1.876)	(0.968)	(0.998)
Observations	1,134	729	729	729	729	729	729
Pseudo R-squared	0.287	0.312	0.292	0.289	0.279	0.284	0.285

Note. Robust standard errors clustered at the courthouse level in parentheses * $p < .10$, ** $p < .05$, *** $p < .01$

From column 1 we can see that, when averaged across the entire sample, the DC7 reforms are not associated with a statistically significant increase in finalisations, although the point estimate is positive. In column 2, where we examine the Sydney DC in isolation, we can see that the DC7 reforms are not associated with a statistically significant increase in finalisations, and the point estimate is negative. In column 3, where we examine the Newcastle DC in isolation, we can see that the DC7 reforms generated an additional 3.34 finalisations each month. In column 4, where we examine the Gosford DC in isolation, we can see that the DC7 reforms are not associated with a significant increase in finalisations, although the point estimate is positive. In column 5, where we examine the Wollongong DC in isolation, we can see that the DC7 reforms are associated with an increase of 5.26 finalisations per month.

We advise some caution when interpreting these estimates. The first reason is the significant differences between each court and the control courts as identified by Figure 2. The second reason is that some of these estimates are not robust to alternative specifications. In Table A1 of the Appendix, we report the results from three robustness checks for each specification in Table 3. These robustness checks include estimating equation (1) using a linear regression; using a quarterly level of aggregation; and including a set of courthouse-specific linear trends. The estimate for the Wollongong DC is largely robust to a linear specification and a quarterly level of aggregation, but inclusion of a set of court-specific linear trends reduces the size of this estimate by around 50 per cent. The estimates for Newcastle are robust to the alternative specifications but are no longer significant when including court-specific linear trends.

¹⁵ A plot analogous to Figure 1 is reported in Figure A1 of the Appendix to investigate the validity of the common trends assumption for each courthouse.

In columns 6 and 7, where we respectively, examine the Lismore and Coffs Harbour DCs in isolation, we can see that the DC7 reforms did not increase the monthly count of finalisations. In fact, the point estimate in both specifications is negative and statistically insignificant (Table 3). Again, we suggest interpreting these estimates with caution; the estimates for Lismore are all statistically significant when using a linear specification, when we aggregate the data at the quarterly level and when we include court-specific linear trends. The estimate for Coffs Harbour is statistically significant when including court-specific linear trends.

By-and-large, the estimates reported in Table 3 are a far cry from the increase of 8.5 cases per month per judge reported by Thorburn and Weatherburn (2018). The simple difference-in-means tests reported in Table 2 (which solely exploit time series variation, like Thorburn and Weatherburn, 2018) enable us to rule out a difference in identification strategy. Said differently, if there was an effect anywhere near the size of that reported by Thorburn and Weatherburn (2018), we would have observed some indication of it in Table 2. The next section explores why the estimates in Table 3 are not as large as those reported by Thorburn and Weatherburn (2018).

Mechanisms for the lower than expected increase

In this section we explore three explanations for why the effect of the DC7 reforms is not as large as expected. The first is that the new (permanent) judges were used to reduce the court’s reliance on acting judges. The second is that the composition of matters committed to the DC has changed after the introduction of the reforms (i.e., there are more trials). The third is that there was an increase in complex trials which take longer to finalise in the courts.

Table 4. Effect of the DC7 reforms on the monthly count of finalisations excluding acting judge cases

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All courts	Sydney	Newcastle	Gosford	Wollongong	Lismore	Coffs Harbour
Estimate	3.277	1.526	3.161	3.590	4.706*	-0.666	0.187
Std. Err.	(3.541)	(3.511)	(2.317)	(2.332)	(2.550)	(1.523)	(1.635)
Observations	1,134	729	729	729	729	729	729
Pseudo R-squared	0.277	0.300	0.282	0.273	0.270	0.276	0.277

Note. Robust standard errors clustered at the courthouse level in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 4 explores the first possibility by excluding cases finalised by an acting judge from the estimation sample entirely. If it is true that the new judges were used to reduce the court’s reliance on acting judges, we would expect the estimates in Table 4 to be *larger* than their counterparts in Table 3. To understand why, consider the simplest possible difference-in-differences model which has two time periods (pre and post policy) and two groups (treatment and control) for a total of four observations: 1) treatment group, pre-policy; 2) treatment group, post-policy; 3) control group, pre-policy; and 4) control group, post-policy. Further suppose that the new judges were used exclusively to replace acting judges on a 1:1 basis. In this case, there would be a downward shift in the count of finalisations for observations (1), (3) and (4). The count of finalisations for observation (2), however, would remain stable. In this case, relative to the estimates reported in Table 3, we would expect a larger coefficient associated with the DC7 reforms.

Table 4 follows an identical layout to Table 3. In columns 1, 2 and 4 we can clearly see larger point estimates. Although the point estimate in column 3 of Table 4 is not larger than its counterpart in Table 3, it is very close in magnitude. This indicates that the additional judges assigned to the Sydney, Newcastle, Gosford and Wollongong DCs were indeed used to alleviate reliance on acting judges. While the point estimate for Coffs Harbour increases and the estimates for the Lismore decrease compared to their

counterparts in Table 3, neither is statistically significant, and in both cases, it is fewer than one finalisation per month. Given the uncertainty around these point estimates (i.e., the large standard errors), it is difficult to draw any definitive conclusion one way or the other.¹⁶

In Table 5 we explore the second possibility (i.e., that there are now more trials in the treatment courthouses, and since trials take longer to finalise, there are fewer finalisations per month). In order to investigate this idea, we use the (raw) disaggregated version of the dataset described earlier and estimate a micro-level model. That is, we estimate a Probit regression that predicts the probability that an *individual case* will be finalised through a trial using a variety of case characteristics.

More formally, we are estimating the following Probit regression:

$$trial_{ict} = \alpha + \delta post_{it} + \gamma X'_i + \theta_c + e_{ict}$$

where $trial_{ict}$ is a binary variable equal to one if case i , finalised in courthouse c , during month-year t was finalised through a trial, zero otherwise. $post_{it}$ is a binary variable equal to one for cases finalised after the introduction of the DC7 reforms, zero otherwise. X'_i is set of case level control variables that includes: sex; age; age at first contact with the criminal justice system; Aboriginality; the ANZSOC code associated with the most serious offence; and the number of prior court appearances (with a proven offence) and prison sentences. θ_c is a set of court fixed effects (used only in column 1 of Table 5; columns 2 – 7 examine one court at a time) and finally, e_{ict} is the error term. The coefficient of interest, δ , can be interpreted as the change in the probability of a trial after the introduction of the reforms, net of controls and fixed effects. Note that this is purely a descriptive exercise; δ does not have a causal interpretation as other factors (e.g., the EAGP and Table reforms) have not been accounted for in this simple regression.

The idea here is to see whether the proportion of cases finalised through trials has increased after the policy date. If it has, then this may explain why there are fewer finalisations than expected.

Table 5. Case level probability of a trial before vs. after the reforms

	(1) All courts	(2) Sydney	(3) Newcastle	(4) Gosford	(5) Wollongong	(6) Lismore	(7) Coffs Harbour
Estimate	-0.036***	-0.027***	-0.002	0.004	-0.021	-0.017	-0.061
Std. Err.	(0.006)	(0.009)	(0.016)	(0.027)	(0.025)	(0.030)	(0.040)
Observations	21,056	9,050	2,068	978	883	826	361
Pseudo R-squared	0.113	0.109	0.133	0.141	0.147	0.117	0.197

Note. Robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$

From Table 5 we can see that the overall probability of a trial (i.e., column 1) decreased after the reforms. We can also see that this decrease is predominately driven by a reduction in the probability of a trial in the Sydney DC (i.e., column 2). We can also see negative, albeit statistically insignificant, estimates for the Newcastle, Wollongong, Lismore and Coffs Harbour DCs. By-and-large, the evidence reported in Table 5 is entirely inconsistent with the argument that an increase in the number of trials is responsible for the relatively small effects reported in Table 3.

Our third possibility is that case complexity has increased in some of our courts. A court with more complex trials has less capacity to finalise other matters. While we cannot determine from the data which trials are likely to be 'complex', trials involving sexual assaults and related offences are known to take longer than others (Wan & Weatherburn, 2017). We repeat the probit analysis above to estimate the likelihood that our treatment courts had an increase in trials involving sexual assault or related matters in the post-period.

¹⁶ Interested readers are directed to Table A2 in the Appendix for a set of robustness checks analogous to those described earlier and reported in Table A1 of the Appendix.

Table 6 presents the results of the probit analysis repeated where the outcome is the probability of a trial involving sexual assaults and related offences. Column 1 indicates that overall there was an increase in the likelihood of a trial involving a sexual assault or related offence of approximately half a percentage point in our treatment courts, compared to before the reforms. This is driven by an increase of approximately one percentage point (an increase of 1 in every 100 matters) in the Sydney DC. We also find that the likelihood of a trial involving sexual assault or a related offence increased in Newcastle and Lismore, although these estimates are statistically insignificant. Overall, the evidence suggests that our failure to detect an increase in finalisations in the Lismore and Sydney DCs may have been caused by an increase in trial complexity. However, this does not explain our negative finding for the Coffs Harbour DC.

Table 6. Case level probability of a trial involving a sexual assault or related offence before vs. after the reforms

	(1) All courts	(2) Sydney	(3) Newcastle	(4) Gosford	(5) Wollongong	(6) Lismore	(7) Coffs Harbour
Estimate	0.005*	0.010**	0.015	-0.009	-0.003	0.021	-0.012
Std. Err.	(0.003)	(0.004)	(0.010)	(0.014)	(0.012)	(0.018)	(0.022)
Observations	21,016	9,077	2,071	1,039	934	834	352
Pseudo R-squared	0.128	0.123	0.155	0.143	0.125	0.0631	0.109

Note. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

DISCUSSION

This study set out to determine the extent to which the appointment of seven additional District Court (DC) judges (i.e., ‘the DC7 reforms’) increased the monthly number of cases finalised in the DC. To answer this question, we compared a treatment group consisting of courthouses that gained at least 10 additional sitting weeks, to courthouses that gained exactly zero sitting weeks, before and after the reforms. Courthouses in the treatment group are the Sydney Downing Centre, Newcastle, Wollongong, Gosford, Lismore and Coffs Harbour DCs.

Using this approach, we found that the DC7 reforms were associated with increases in the monthly number of finalisations in the Newcastle and Wollongong DCs by 3.34 and 5.26, respectively. We also found some (statistically insignificant) evidence that the additional judges were associated with an increase in the monthly count of finalisations in the Gosford DC by 1.26. Interestingly, however, the additional judges did not appear to generate any additional finalisations in the Sydney, Lismore or Coffs Harbour DCs. In order to investigate why this might be the case, we examined three possibilities: first, that the new judges were used to reduce the state’s reliance on acting judges; second, that the composition of cases changed after the reforms (i.e., there are more trials, which take longer to finalise, and thus fewer finalisations); and third, that there are more complex trials (e.g. those involving sexual assault and related offences). We found evidence that the additional judges assigned to the Sydney, Newcastle, Gosford and Wollongong DCs were indeed used to alleviate the court’s reliance on acting judges. We also find some support for the conclusion that the null effect in the Sydney and Lismore DCs was due to an increase in trials involving sexual assault or related offences in these courts.

It should be noted that this analysis suffers from three limitations. First is that we do not find common prior trends between our treatment and control courts which erodes the credibility of the main assumption of our analysis. This was less of a problem for our overall analysis and those relating to the Wollongong, Lismore, and Coffs Harbour DCs. This means that the control courts may not provide a valid counterfactual for the Sydney Downing Centre, Newcastle and Gosford DCs. A further issue is that some of our estimates are not robust to alternative specifications. Given these limitations, our estimates do not have a causal interpretation. Finally, the follow up period for the analysis is relatively short given the time DC matters take to proceed through to finalisation and the nature of the intervention. It is therefore possible that we have not captured the full benefits of the new appointments.

The estimates reported in this bulletin add to the growing body of evidence indicating that recent measures to reduce backlog in the DC are working (see for example, Klauzner & Yeong, 2021; Rahman et al., 2017; Ringland, 2020). This study and prior work by Thorburn and Weatherburn (2018) indicate that appointing new judges can reduce backlog in the DC by increasing finalisations. However, for these appointments to have the largest effect they should add to (rather than replace) existing judicial capacity. Furthermore, additional judges do not impact upon the 'inflow' of cases into the DC, and their effectiveness is sensitive to changes in trial complexity and volume. Efforts to increase effective case management in the DC (e.g. see DCC Practice Notes 18, 19, 20, 21, 22) are ongoing. While the impact of these initiatives could not be considered here (as many fall outside the current study period) they should be the focus of future research.

ACKNOWLEDGEMENTS

We thank Simon Moore and Jenifar Garvey for providing information on the implementation of the DC7. We would also like to thank Suzanne Poynton, Jackie Fitzgerald, Clare Ringland, Alana Cook and an anonymous peer reviewer for their comments on earlier drafts of this bulletin, and Florence Sin for desktop-publishing this report.

REFERENCES

- Australian Bureau of Statistics (2011). *Australian and New Zealand Standard Offence Classifications*. (Cat. No. 1234.0). Retrieved 17 Nov 2020 from <http://www.abs.gov.au/ausstats/abs@.nsf/mf/1234.0>
- Garvey, J. (2020). *Personal communication*, 20th of November 2020.
- Judicial Commission. Retrieved 5 Aug 2020 from: <https://www.judcom.nsw.gov.au/education/#:~:text=Home%20Judicial%20Education-Judicial%20Education,to%20the%20highest%20professional%20standard.&text=From%20there%2C%20we%20aim%20to,court%20procedure%20and%20community%20values.>
- Klauzner, I., & Yeong, S. (2021). *The impact of the Early Appropriate Guilty Plea reforms on guilty pleas, time to justice, and District Court finalisations* (Crime and Justice Bulletin No. 240). Sydney: NSW Bureau of Crime Statistics and Research.
- NSW Auditor-General (2019). *Managing growth in the NSW prison population*. Retrieved 5 Aug 2020 from: <https://www.audit.nsw.gov.au/our-work/reports/managing-growth-in-the-nsw-prison-population>.
- NSW Bureau of Crime Statistics and Research (2020). *NSW Criminal Courts Statistics 2019*. Retrieved 29 Mar 2020 from: https://www.bocsar.nsw.gov.au/Pages/bocsar_publication/Pub_Summary/CCS-Annual/Criminal-Court-Statistics-Dec-2019.aspx
- NSW Department of Communities & Justice. (2020). District Court website. Retrieved 5 Aug 2020 from: http://www.districtcourt.justice.nsw.gov.au/Pages/courtlists/sitting_dates_crim.aspx
- NSW Government (2018). *Seven extra judges to ease District Court burden*. Retrieved 5 Aug 2020 from: <https://www.justice.nsw.gov.au/Documents/Media%20Releases/2018/seven-extra-judges-to-ease-district-court-burden.pdf>
- Rahman, S., Poynton, S., & Weatherburn, D. (2017). *The NSW Rolling List Court Evaluation: Final Report* (Crime and Justice Bulletin No. 208). Sydney: NSW Bureau of Crime Statistics and Research.
- Ringland, C. (2020). *Evaluating the first tranche of the Table Offences Reform: Impacts on District Court finalisations, time to finalisation and sentencing outcomes* (Crime and Justice Bulletin No. 231). Sydney: NSW Bureau of Crime Statistics and Research.
- Ringland, C. (2021). *The second tranche of the Table Offences Reform: Impacts on District and Local Court finalisations, time to finalisation and sentencing outcomes*. (Bureau Brief No. 156). Sydney: NSW Bureau of Crime Statistics and Research.
- The Public Defenders. (2020). *Annual Review 2018-2019*. Retrieved 5 Aug 2020 from: <https://www.publicdefenders.nsw.gov.au/Documents/Public%20Defenders%20-%20Annual%20Report%202018-19.pdf>
- Thorburn, H., & Weatherburn, D. (2018). *An evaluation of measures taken to increase finalisations in the NSW District Criminal Court* (Crime and Justice Bulletin No. 217). Sydney: NSW Bureau of Crime Statistics and Research.
- Wan, W., & Weatherburn, D. (2017). *The determinants of trial duration: A preliminary study* (Bureau Brief 127). Sydney: NSW Bureau of Crime Statistics and Research.

APPENDIX

Figure A1. Monthly finalisations for each treatment court vs. all control courts

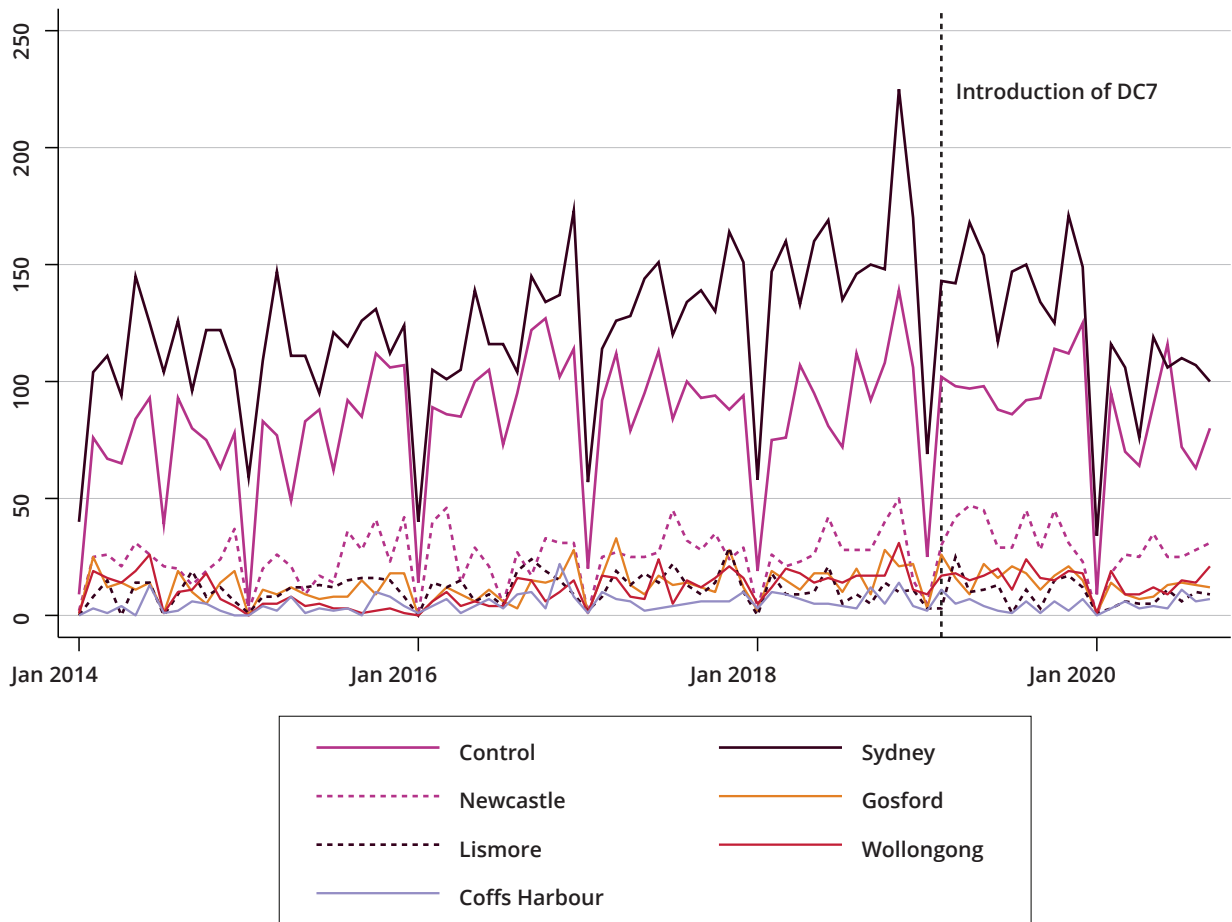


Table A1. Negative binomial and linear regression difference-in-differences estimates for courts who experienced a 25 per cent or greater increase in sitting weeks

	(1) Negative binomial	(2) Linear regression
All courts	1.154	-0.334
Std. err.	(2.006)	(1.121)
Observations	1,458	1,458
Pseudo R-squared	0.269	0.916
Moree	-0.803	-1.359
Std. err.	(1.047)	(0.933)
Observations	729	729
Pseudo R-squared	0.847	0.286
Coffs Harbour	-1.297	-0.864
Std. err.	(1.047)	(0.998)
Observations	729	729
Pseudo R-squared	0.844	0.285
Lismore	-2.432**	-1.539
Std. err.	(1.047)	(0.968)
Observations	729	729
Pseudo R-squared	0.84	0.284
Sydney	-1.099	-0.331
Std. err.	(1.047)	(2.343)
Observations	729	729
Pseudo R-squared	0.922	0.312
Wollongong	3.688***	5.262***
Std. err.	(1.047)	(1.876)
Observations	729	729
Pseudo R-squared	0.837	0.279
Orange	-0.808	-0.688
Std. err.	(1.047)	(1.053)
Observations	729	729
Pseudo R-squared	0.846	0.275
Tamworth	0.858	4.367***
Std. err.	(1.047)	(1.509)
Observations	729	729
Pseudo R-squared	0.845	0.284
Gosford	0.163	1.260
Std. err.	(1.047)	(1.334)
Observations	729	729
Pseudo R-squared	0.839	0.289
Goulburn	-1.314	-1.159
Std. err.	(1.047)	(1.118)
Observations	729	729
Pseudo R-squared	0.844	0.278
Albury	-0.293	0.867
Std. err.	(1.047)	(1.107)
Observations	729	729
Pseudo R-squared	0.844	0.273

* $p < .10$, ** $p < .05$, *** $p < .01$

Table A2. Robustness to linear specification, quarterly level of aggregation and the inclusion of court-specific linear trends, all cases

	(1) Linear regression	(2) Quarterly	(3) Linear trend
All courts	0.706	4.993	2.739
Std. err.	(1.589)	(7.860)	(5.161)
Observations	1134	378	1134
Pseudo R-squared	0.914	0.297	0.295
Coffs Harbour	-1.297	-2.004	-3.817***
Std. err.	(1.047)	(2.489)	(1.345)
Observations	729	243	729
Pseudo R-squared	0.844	0.294	0.294
Gosford	0.163	3.325	0.952
Std. err.	(1.047)	(3.261)	(3.002)
Observations	729	243	729
Pseudo R-squared	0.839	0.293	0.298
Lismore	-2.432**	-7.888***	-0.364***
Std. err.	(1.047)	(2.260)	(0.130)
Observations	729	243	729
Pseudo R-squared	0.84	0.295	0.293
Newcastle	5.214***	9.098**	5.832
Std. err.	(1.047)	(4.068)	(4.361)
Observations	729	243	729
Pseudo R-squared	0.838	0.297	0.301
Sydney	-1.099	1.862	2.171
Std. err.	(1.047)	(6.450)	(6.833)
Observations	729	243	729
Pseudo R-squared	0.922	0.316	0.321
Wollongong	3.688***	15.701***	2.535
Std. err.	(1.047)	(4.690)	(3.032)
Observations	729	243	729
Pseudo R-squared	0.837	0.274	0.287

Note. Robust standard errors clustered at the courthouse level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3. Robustness to linear specification, quarterly level of aggregation and inclusion of court-specific linear trends, all cases not finalised by an acting judge

	(1) Linear specification	(2) Quarterly	(3) Linear trend
All courts	1.887	10.066	3.435
Std. err.	(1.301)	(11.261)	(5.249)
Observations	1,134	378	1,134
Pseudo R-squared	0.904	0.275	0.287
Coffs Harbour	-0.482	1.227	-2.527
Std. err.	(0.930)	(5.085)	(1.664)
Observations	729	243	729
Pseudo R-squared	0.841	0.265	0.292
Gosford	2.054*	10.516	1.754
Std. err.	(0.930)	(6.916)	(3.178)
Observations	729	243	729
Pseudo R-squared	0.833	0.26	0.287
Lismore	-1.426	-5.077	1.439
Std. err.	(0.930)	(4.080)	(2.934)
Observations	729	243	729
Pseudo R-squared	0.836	0.272	0.291
Newcastle	4.376***	9.022	4.879
Std. err.	(0.930)	(7.260)	(4.295)
Observations	729	243	729
Pseudo R-squared	0.832	0.272	0.298
Sydney	3.722***	8.271	3.052
Std. err.	(0.930)	(11.942)	(6.665)
Observations	729	243	729
Pseudo R-squared	0.914	0.288	0.315
Wollongong	3.078**	17.288**	2.312
Std. err.	(0.930)	(8.399)	(3.199)
Observations	729	243	729
Pseudo R-squared	0.833	0.252	0.283

Note. Robust standard errors clustered at the courthouse level in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A4. Event study estimates, all courts

	(1) All courts	(2) Sydney	(3) Newcastle	(4) Gosford	(5) Wollongong	(6) Lismore	(7) Coffs Harbour
Lag 61	-6.000 (7.615)	-27.000*** (1.111)	-1.000 (1.111)	-1.000 (1.111)	-6.000*** (1.111)	-1.000 (1.111)	0.000 (1.111)
Lag 60	9.125 (7.615)	28.625*** (5.660)	13.625** (5.660)	15.625** (5.660)	3.625 (5.660)	-1.375 (5.660)	-5.375 (5.660)
Lag 59	9.750 (7.615)	36.750*** (4.702)	15.750** (4.702)	3.750 (4.702)	1.750 (4.702)	6.750 (4.702)	-6.250 (4.702)
Lag 58	4.333 (7.615)	20.000*** (3.930)	11.000** (3.930)	6.000 (3.930)	0.000 (3.930)	-8.000* (3.930)	-3.000 (3.930)
Lag 57	14.125* (7.615)	68.625*** (6.757)	18.625** (6.757)	0.625 (6.757)	2.625 (6.757)	3.625 (6.757)	-9.375 (6.757)
Lag 56	12.667* (7.615)	47.500*** (9.815)	12.500 (9.815)	2.500 (9.815)	8.500 (9.815)	2.500 (9.815)	2.500 (9.815)
Lag 55	4.417 (7.615)	33.250*** (1.693)	14.250*** (1.693)	-3.750* (1.693)	-9.750*** (1.693)	-4.750** (1.693)	-2.750 (1.693)
Lag 54	7.333 (7.615)	48.500*** (7.601)	6.500 (7.601)	7.500 (7.601)	-7.500 (7.601)	-2.500 (7.601)	-8.500 (7.601)
Lag 53	3.792 (7.615)	20.125** (7.336)	1.125 (7.336)	0.125 (7.336)	-4.875 (7.336)	9.125 (7.336)	-2.875 (7.336)
Lag 52	8.083 (7.615)	46.750*** (3.198)	7.750** (3.198)	-4.250 (3.198)	2.750 (3.198)	-1.250 (3.198)	-3.250 (3.198)
Lag 51	10.250 (7.615)	48.250*** (4.604)	14.250** (4.604)	6.250 (4.604)	-6.750 (4.604)	4.250 (4.604)	-4.750 (4.604)
Lag 50	6.708 (7.615)	29.375*** (5.920)	25.375*** (5.920)	9.375 (5.920)	-11.625* (5.920)	-3.625 (5.920)	-8.625 (5.920)
Lag 49	-2.208 (7.615)	-7.375*** (1.526)	-0.375 (1.526)	-0.375 (1.526)	-6.375*** (1.526)	0.625 (1.526)	0.625 (1.526)
Lag 48	3.750 (7.615)	32.750*** (4.930)	7.750 (4.930)	0.750 (4.930)	-11.250* (4.930)	-2.250 (4.930)	-5.250 (4.930)
Lag 47	11.167 (7.615)	71.500*** (5.471)	14.500** (5.471)	-0.500 (5.471)	-10.500* (5.471)	-1.500 (5.471)	-6.500 (5.471)
Lag 46	10.500 (7.615)	39.000*** (1.936)	13.000*** (1.936)	6.000** (1.936)	-4.000* (1.936)	6.000** (1.936)	3.000 (1.936)
Lag 45	1.917 (7.615)	34.750*** (6.199)	-3.250 (6.199)	-1.250 (6.199)	-12.250* (6.199)	1.750 (6.199)	-8.250 (6.199)
Lag 44	0.292 (7.615)	18.125** (6.234)	4.125 (6.234)	-3.875 (6.234)	-11.875* (6.234)	2.125 (6.234)	-6.875 (6.234)
Lag 43	6.875 (7.615)	47.375*** (2.985)	4.375 (2.985)	0.375 (2.985)	-10.625*** (2.985)	4.375 (2.985)	-4.625 (2.985)
Lag 42	6.458 (7.615)	37.625*** (5.446)	22.625*** (5.446)	-3.375 (5.446)	-14.375** (5.446)	3.625 (5.446)	-7.375 (5.446)
Lag 41	8.333 (7.615)	49.500*** (4.828)	15.500** (4.828)	4.500 (4.828)	-15.500** (4.828)	5.500 (4.828)	-9.500* (4.828)
Lag 40	8.792 (7.615)	51.125*** (6.138)	25.125*** (6.138)	-4.875 (6.138)	-17.875** (6.138)	2.125 (6.138)	-2.875 (6.138)

Table A4. Event study estimates, all courts - continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All courts	Sydney	Newcastle	Gosford	Wollongong	Lismore	Coffs Harbour
Lag 39	4.542 (7.615)	32.875*** (5.007)	7.875 (5.007)	4.875 (5.007)	-16.125** (5.007)	1.875 (5.007)	-4.125 (5.007)
Lag 38	7.417 (7.615)	44.750*** (5.615)	26.750*** (5.615)	4.750 (5.615)	-18.250** (5.615)	-5.250 (5.615)	-8.250 (5.615)
Lag 37	-6.958 (7.615)	-27.625*** (1.480)	-3.625** (1.480)	-1.625 (1.480)	-7.625*** (1.480)	-1.625 (1.480)	0.375 (1.480)
Lag 36	6.000 (7.615)	28.000*** (4.090)	27.000*** (4.090)	-5.000 (4.090)	-11.000** (4.090)	3.000 (4.090)	-6.000 (4.090)
Lag 35	8.542 (7.615)	24.375*** (4.050)	33.375*** (4.050)	1.375 (4.050)	-6.625 (4.050)	1.375 (4.050)	-2.625 (4.050)
Lag 34	2.000 (7.615)	28.500*** (4.233)	1.500 (4.233)	-1.500 (4.233)	-12.500** (4.233)	4.500 (4.233)	-8.500* (4.233)
Lag 33	7.125 (7.615)	60.625*** (5.959)	14.625** (5.959)	-6.375 (5.959)	-12.375* (5.959)	-6.375 (5.959)	-7.375 (5.959)
Lag 32	2.833 (7.615)	37.000*** (5.206)	6.000 (5.206)	-2.000 (5.206)	-15.000** (5.206)	-4.000 (5.206)	-5.000 (5.206)
Lag 31	1.833 (7.615)	41.000*** (4.766)	-6.000 (4.766)	-3.000 (4.766)	-11.000** (4.766)	-5.000 (4.766)	-5.000 (4.766)
Lag 30	5.750 (7.615)	26.250*** (5.288)	13.250** (5.288)	-8.750 (5.288)	-1.750 (5.288)	7.250 (5.288)	-1.750 (5.288)
Lag 29	10.375 (7.615)	63.875*** (7.862)	-0.125 (7.862)	-0.125 (7.862)	-6.125 (7.862)	8.875 (7.862)	-4.125 (7.862)
Lag 28	6.917 (7.615)	52.250*** (6.359)	15.250** (6.359)	-1.750 (6.359)	-15.750** (6.359)	3.250 (6.359)	-11.750 (6.359)
Lag 27	13.708* (7.615)	58.375*** (7.090)	16.375** (7.090)	3.375 (7.090)	-8.625 (7.090)	2.375 (7.090)	10.375 (7.090)
Lag 26	17.708** (7.615)	92.875*** (7.095)	14.875* (7.095)	13.875* (7.095)	-5.125 (7.095)	-5.125 (7.095)	-5.125 (7.095)
Lag 25	-4.042 (7.615)	-11.375*** (1.089)	-3.375** (1.089)	-1.375 (1.089)	-7.375*** (1.089)	-0.375 (1.089)	-0.375 (1.089)
Lag 24	8.292 (7.615)	36.625*** (5.421)	11.625* (5.421)	5.625 (5.421)	-0.375 (5.421)	-3.375 (5.421)	-0.375 (5.421)
Lag 23	11.958 (7.615)	46.125*** (6.354)	11.125 (6.354)	19.125** (6.354)	-3.875 (6.354)	5.125 (6.354)	-5.875 (6.354)
Lag 22	10.250 (7.615)	52.250*** (4.999)	13.250** (4.999)	3.250 (4.999)	-7.750 (4.999)	3.250 (4.999)	-2.750 (4.999)
Lag 21	10.250 (7.615)	66.250*** (5.187)	11.250* (5.187)	-2.750 (5.187)	-10.750* (5.187)	6.250 (5.187)	-8.750 (5.187)
Lag 20	13.167* (7.615)	71.000*** (7.052)	11.000 (7.052)	3.000 (7.052)	4.000 (7.052)	0.000 (7.052)	-10.000 (7.052)
Lag 19	12.292 (7.615)	43.625*** (5.619)	32.625*** (5.619)	2.625 (5.619)	-11.375* (5.619)	11.625* (5.619)	-5.375 (5.619)
Lag 18	10.958 (7.615)	55.625*** (5.077)	17.625*** (5.077)	1.625 (5.077)	-3.375 (5.077)	0.625 (5.077)	-6.375 (5.077)
Lag 17	10.667 (7.615)	61.500*** (5.162)	14.500** (5.162)	0.500 (5.162)	-5.500 (5.162)	-2.500 (5.162)	-4.500 (5.162)

Table A4. Event study estimates, all courts - continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All courts	Sydney	Newcastle	Gosford	Wollongong	Lismore	Coffs Harbour
Lag 16	11.375 (7.615)	52.375*** (3.764)	21.375*** (3.764)	-1.625 (3.764)	-1.625 (3.764)	2.375 (3.764)	-4.625 (3.764)
Lag 15	22.292*** (7.615)	87.125*** (4.115)	11.125** (4.115)	17.125*** (4.115)	4.125 (4.115)	18.125*** (4.115)	-3.875 (4.115)
Lag 14	14.208* (7.615)	73.375*** (5.692)	15.375** (5.692)	0.375 (5.692)	-1.625 (5.692)	-1.625 (5.692)	-0.625 (5.692)
Lag 13	-2.583 (7.615)	-10.250*** (0.821)	-1.250 (0.821)	-0.250 (0.821)	-3.250*** (0.821)	-2.250** (0.821)	1.750* (0.821)
Lag 12	17.250** (7.615)	71.750*** (2.668)	14.750*** (2.668)	9.750*** (2.668)	-3.250 (2.668)	8.750** (2.668)	1.750 (2.668)
Lag 11	17.458** (7.615)	84.625*** (4.067)	9.625** (4.067)	5.625 (4.067)	4.625 (4.067)	-0.375 (4.067)	0.625 (4.067)
Lag 10	8.083 (7.615)	53.750*** (5.335)	7.750 (5.335)	-2.250 (5.335)	-1.250 (5.335)	-4.250 (5.335)	-5.250 (5.335)
Lag 9	14.917* (7.615)	82.250*** (4.741)	12.250** (4.741)	6.250 (4.741)	-3.750 (4.741)	-1.750 (4.741)	-5.750 (4.741)
Lag 8	23.000*** (7.615)	93.000*** (3.800)	30.000*** (3.800)	8.000* (3.800)	0.000 (3.800)	11.000** (3.800)	-4.000 (3.800)
Lag 7	11.625 (7.615)	60.125*** (4.378)	17.125*** (4.378)	1.125 (4.378)	-0.875 (4.378)	-3.875 (4.378)	-3.875 (4.378)
Lag 6	11.125 (7.615)	66.125*** (5.440)	12.125* (5.440)	6.125 (5.440)	-2.875 (5.440)	-4.875 (5.440)	-9.875 (5.440)
Lag 5	13.292* (7.615)	72.625*** (4.832)	14.625** (4.832)	-2.375 (4.832)	-0.375 (4.832)	-6.375 (4.832)	1.625 (4.832)
Lag 4	16.458** (7.615)	68.625*** (7.353)	24.625** (7.353)	14.625* (7.353)	-2.375 (7.353)	0.625 (7.353)	-7.375 (7.353)
Lag 3	29.083*** (7.615)	141.750*** (10.446)	30.750** (10.446)	3.750 (10.446)	7.750 (10.446)	-7.250 (10.446)	-2.250 (10.446)
Lag 2	13.542* (7.615)	90.875*** (6.179)	-0.125 (6.179)	8.875 (6.179)	-8.125 (6.179)	-2.125 (6.179)	-8.125 (6.179)
Lead 0	13.542* (7.615)	64.375*** (5.433)	15.375** (5.433)	13.375** (5.433)	-1.625 (5.433)	-9.625 (5.433)	-0.625 (5.433)
Lead 1	17.042** (7.615)	63.875*** (6.238)	27.875*** (6.238)	3.875 (6.238)	-0.125 (6.238)	12.875* (6.238)	-6.125 (6.238)
Lead 2	18.500** (7.615)	90.000*** (5.664)	33.000*** (5.664)	-3.000 (5.664)	-3.000 (5.664)	-2.000 (5.664)	-4.000 (5.664)
Lead 3	17.875** (7.615)	75.875*** (6.108)	30.875*** (6.108)	9.875 (6.108)	-1.125 (6.108)	-1.125 (6.108)	-7.125 (6.108)
Lead 4	9.792 (7.615)	40.125*** (5.093)	16.125** (5.093)	5.125 (5.093)	3.125 (5.093)	2.125 (5.093)	-7.875 (5.093)
Lead 5	12.208 (7.615)	70.375*** (6.114)	16.375** (6.114)	10.375 (6.114)	-5.625 (6.114)	-9.625 (6.114)	-8.625 (6.114)
Lead 6	18.792** (7.615)	72.625*** (6.316)	31.625*** (6.316)	6.625 (6.316)	6.625 (6.316)	-0.375 (6.316)	-4.375 (6.316)
Lead 7	8.500 (7.615)	56.500*** (5.237)	14.500** (5.237)	-0.500 (5.237)	-1.500 (5.237)	-8.500 (5.237)	-9.500 (5.237)

Table A4. Event study estimates, all courts - continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All courts	Sydney	Newcastle	Gosford	Wollongong	Lismore	Coffs Harbour
Lead 8	10.875 (7.615)	44.875*** (7.004)	28.875*** (7.004)	2.875 (7.004)	-5.125 (7.004)	0.875 (7.004)	-7.125 (7.004)
Lead 9	17.458** (7.615)	91.125*** (8.972)	15.125 (8.972)	7.125 (8.972)	-0.875 (8.972)	3.125 (8.972)	-10.875 (8.972)
Lead 10	9.667 (7.615)	67.500*** (9.377)	5.500 (9.377)	-0.500 (9.377)	-3.500 (9.377)	-3.500 (9.377)	-7.500 (9.377)
Lead 11	-7.000 (7.615)	-33.000*** (0.932)	-3.000** (0.932)	0.000 (0.932)	-6.000*** (0.932)	0.000 (0.932)	0.000 (0.932)
Lead 12	4.917 (7.615)	38.250*** (6.917)	4.250 (6.917)	2.250 (6.917)	1.250 (6.917)	-8.750 (6.917)	-7.750 (6.917)
Lead 13	6.208 (7.615)	31.375*** (4.464)	15.375*** (4.464)	0.375 (4.464)	-5.625 (4.464)	-2.625 (4.464)	-1.625 (4.464)
Lead 14	0.792 (7.615)	2.125 (3.308)	15.125*** (3.308)	-0.875 (3.308)	-4.875 (3.308)	-2.875 (3.308)	-3.875 (3.308)
Lead 15	7.208 (7.615)	41.875*** (5.506)	21.875*** (5.506)	-3.125 (5.506)	-5.125 (5.506)	-6.125 (5.506)	-6.125 (5.506)
Lead 16	1.292 (7.615)	25.625** (8.588)	8.625 (8.588)	-1.375 (8.588)	-11.375 (8.588)	-3.375 (8.588)	-10.375 (8.588)
Lead 17	9.125 (7.615)	35.125*** (4.092)	14.125*** (4.092)	5.125 (4.092)	0.125 (4.092)	-2.875 (4.092)	3.125 (4.092)
Lead 18	9.750 (7.615)	33.250*** (3.459)	18.250*** (3.459)	5.250 (3.459)	0.250 (3.459)	2.250 (3.459)	-0.750 (3.459)
Lead 19	7.958 (7.615)	24.125*** (5.137)	19.125*** (5.137)	2.125 (5.137)	5.125 (5.137)	-0.875 (5.137)	-1.875 (5.137)
Observations	1134	729	729	729	729	729	729
R-squared	0.924	0.976	0.867	0.850	0.850	0.849	0.850
F-stat (Lags)	1.6	4021.0	350.4	34.2	68.2	164.0	62.6
p-value	0.005	0.000	0.000	0.000	0.000	0.000	0.000

Standard errors in parentheses *** $p < .10$, ** $p < .05$, $p < .01$

NSW BUREAU OF CRIME STATISTICS AND RESEARCH - LEVEL 1, HENRY DEANE BUILDING, 20 LEE STREET, SYDNEY 2000

bcsr@justice.nsw.gov.au • www.bocsar.nsw.gov.au • Ph: (02) 8346 1100 • Fax: (02) 8346 1298

ISSN 2204-5538 (Online) • ISBN 978-1-922576-10-1

© State of New South Wales through the Department of Communities and Justice 2021. You may copy, distribute, display, download and otherwise freely deal with this work for any purpose, provided that you attribute the Department of Communities and Justice as the owner. However, you must obtain permission if you wish to (a) charge others for access to the work (other than at cost), (b) include the work in advertising or a product for sale, or (c) modify the work.