Technical Appendix (for Part 2)

Ralf Steinhauser and Robert Breunig

24 March 2020

1. Introduction

This appendix is constructed as follows. First, more details are provided on the Corelogic data. Then differences between the Corelogic data and the ACT administrative data on the number of property sales are then discussed. Additional detail on the rental bonds data used is also provided. Finally, the four different control groups (counterfactuals) used in the analyses are described alongside the test results of the analysis of the common trends assumption. Extended results tables for all tables in the main report, including all control groups, are provided in the second to last section and results using administrative data from NSW and ACT in an alternative data approach are provided at the end of this appendix.

2. Corelogic data

CoreLogic is the largest provider of property information in Australia.[1] CoreLogic accesses a range of government data relating to property transactions, primarily through the Valuer Generals of each State and Territory. We have ten years of Corelogic property sales data available for our analysis. This includes three years before the ACT tax reform and seven years after the reform from late 2009 until late 2019. The data is a panel of suburb level, monthly statistics. We observe, for each property type (Houses or Units), the number of properties sold in the suburb, the median sale price for the suburb and the median value of advertised weekly rents in the suburb.

We observe over 10,000 suburbs[2] across all states and territories in the data. There are a total of 770,000 suburb-month observations where at least one property sold. Two thirds of these are observations on house sales and one third are observations on unit sales.

Figure 1 gives an overview of house price trends by State in the CoreLogic data. The price presented is a suburb-level median house price over 12 months. This allow us to focus on the longer term trend and minimise seasonal volatility. We observe a general upward trend in the

[1] https://www.corelogic.com.au/about-us

[2] Suburb is the smallest geography region in the ASGC geography hierarchy.

house price data. In nominal terms, in some States, the average median house price grew close to 40% over this short period.[3]

Figure 1. Average median house price trends by State and Territory 2010-2019

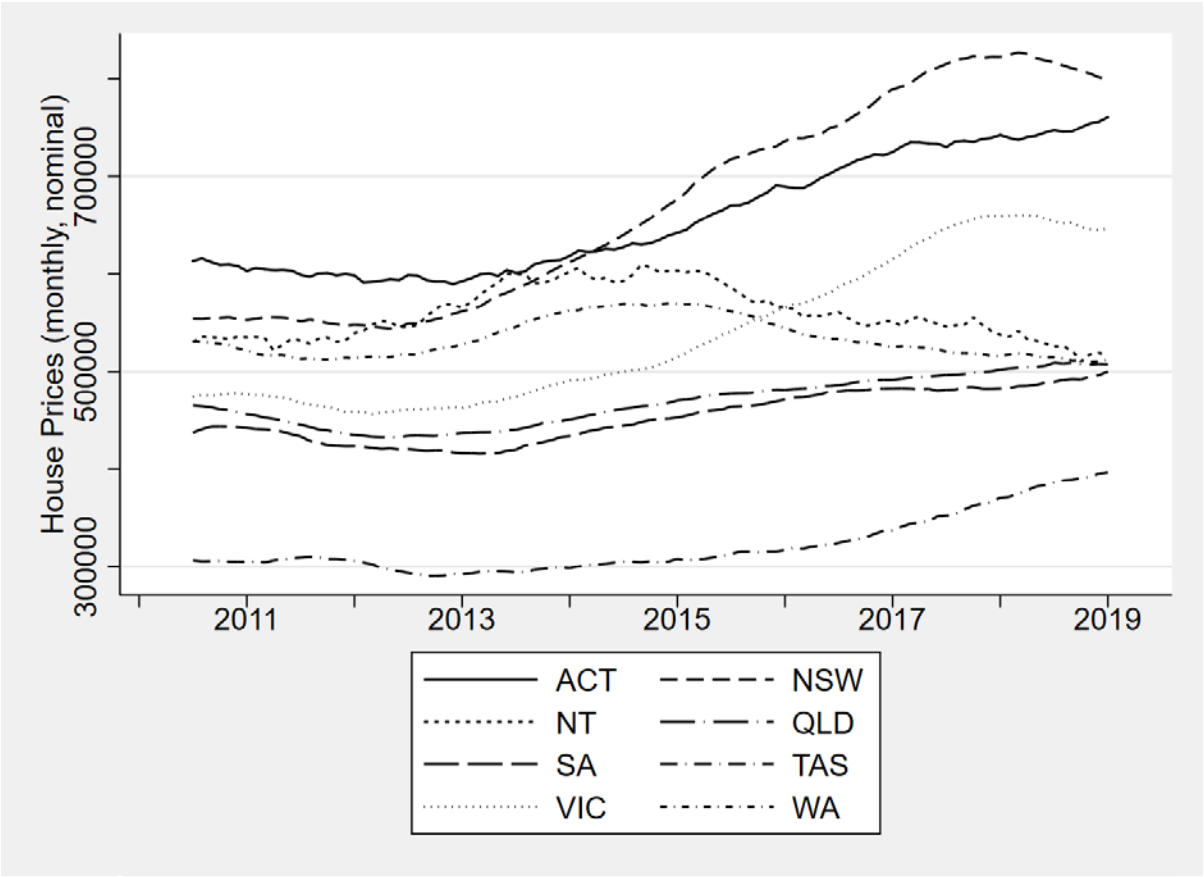
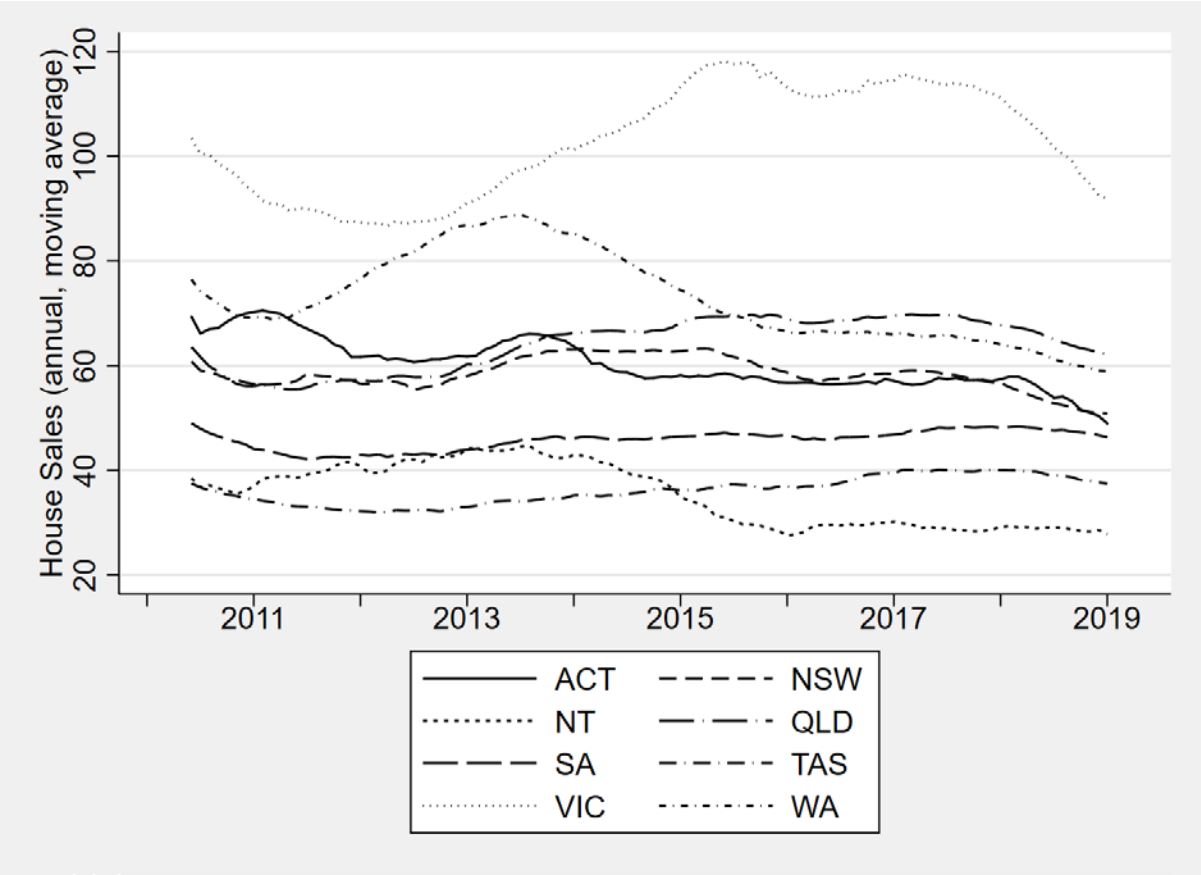


Figure 2 gives an overview of state and territories' average annual suburb sale numbers over time. A moving average is calculated to focus on the longer-term trend without seasonal fluctuations. We observe a largely sideways movement with some larger upturns and downturns in some states. Victoria and NSW have greater volatility in the number of sales over time.

According to the Corelogic data, the number of house sales per suburb in the ACT has largely moved sideways since 2012. This trend is also present for unit sales. These findings seem to be at odds with the administrative records from the ACT. We explore this further in the next subsection.

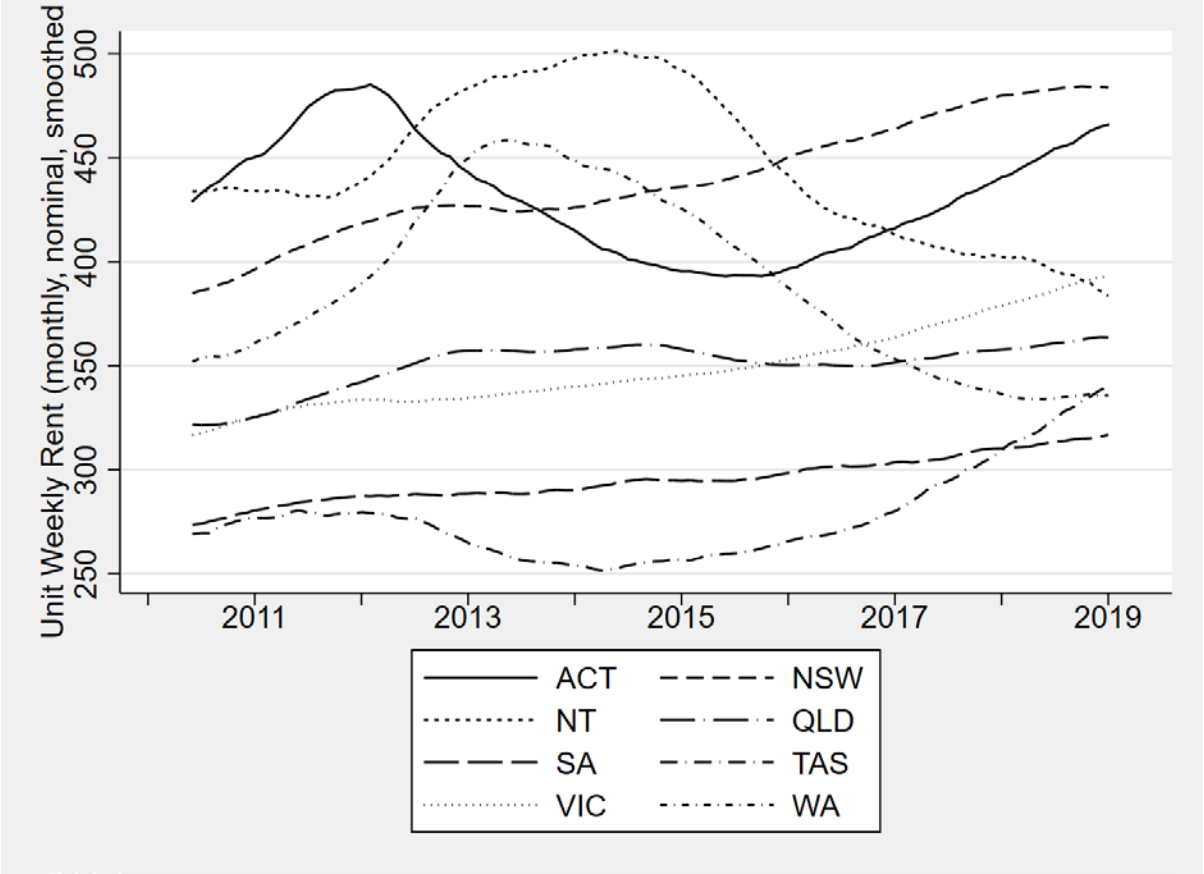
[3] Note we are not concerned with deflating sale prices for our econometric analysis as we employ time specific effects that will account for national inflation. The ABS does not provide State level inflation series. One other avenue we have not taken is to use a price index of capital cities.

Figure 2. Average number of houses sold, by State and Territory 2010-2019



As a final visualization of the Corelogic data we plot rental prices. Figure 3 presents the average unit's weekly rental price across the different states over time from 2010 to 2019. The price presented is an average over 12 months in order to minimise seasonal variation. We observe a general upward trend in nominal rental prices across most states. The notable exceptions are in TAS NT, WA and the ACT, where significant downturns arise within our sample period. Specifically, for the ACT we observe a downturn starting in 2012 which lasts for about 4 years. Rental prices in the ACT only return to pre-2012 levels in 2019. This broadly coincides with the property tax reform. It is likely that the underlying factors driving this change will have an effect on the reform estimates. This same trend is visible in the ACT's rental bond data.

Figure 3. Average weekly unit rental price, by State and Territory 2010-2019



3. Administrative Data for NSW and ACT

As a secondary data source to Corelogic we use administrative sales data from the ACT and NSW. This section gives a brief overview over the data.

We use fifteen years of administrative property sales data which were available for our analysis. This includes eight years before the ACT tax reform and seven years after the reform from 2004/05 until 2018/19. The data are individual sales in this period which we aggregate to a panel by postcode and month. The final panel has for each property type (Houses or Units), the number of properties sold in the postcode and the mean sale price in that postcode. There are no data on rental payments. Note that aggregating by postcode means that the data represent larger geographical areas than the suburb-level Corelogic data.

We observe over 674 postcodes across the ACT and NSW. There are a total of 171,000 postcode-month observations where at least one property was sold. Similar to the Corelogic data, two-thirds of the observations represent house sales and the remaining third are unit sales.

Figure 4 gives an overview of house price trends by state/territory in the administrative data. The price presented is an average house price over 12 months to focus on the longer term trend and minimise seasonal volatility. We observe a general upward trend in the house price

data. In nominal terms, the average house price grew close to 55% in the ACT and 130% in NSW over this period.

Figure 4 Average house price trends ACT and NSW 2005-2019

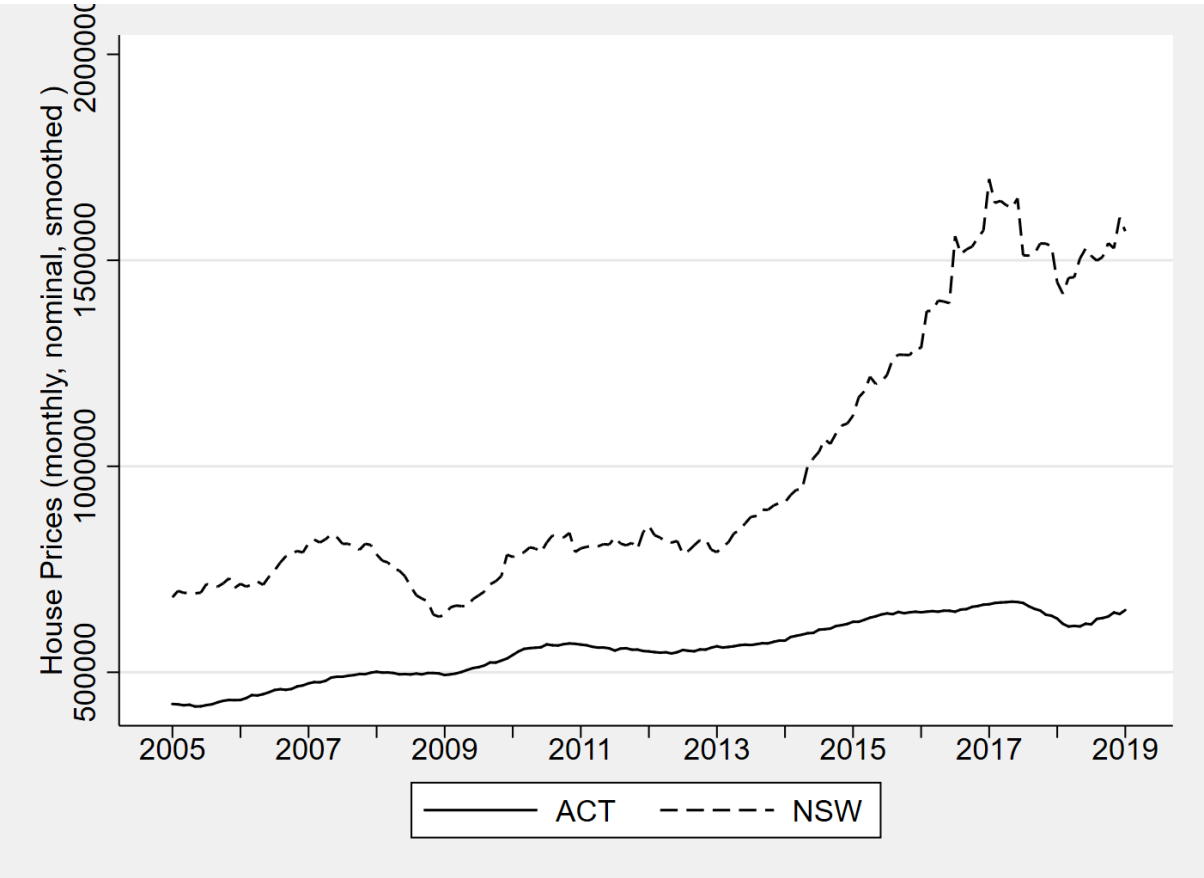
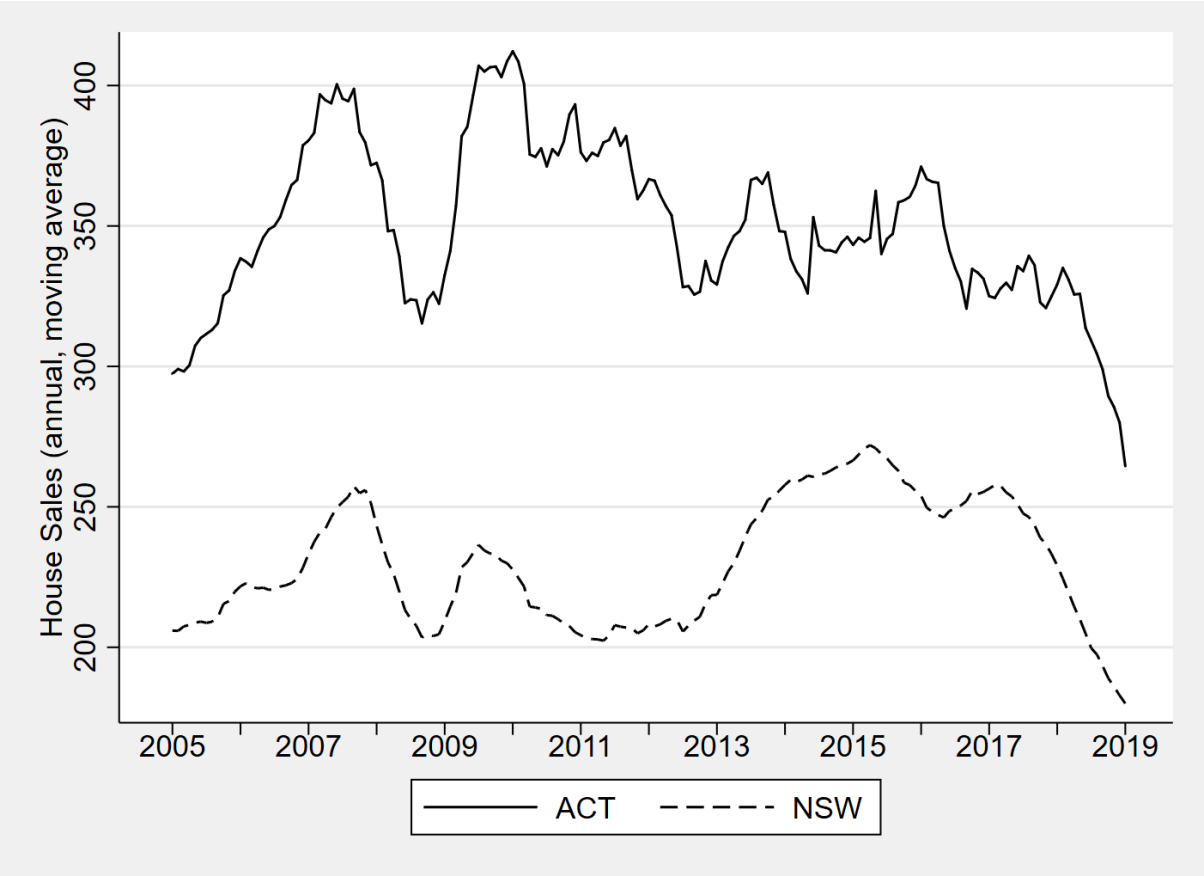


Figure 5 gives an overview of NSW and ACT average annual postcode level sale numbers over time. A moving, 12-monthaverage is calculated to focus on the longer-term trend without seasonal fluctuations. Even with the 12-month moving average, we observe large fluctuations in sales volumes over time.

Figure 5 Average number of houses sold in each postcode, ACT and NSW 2005-2019



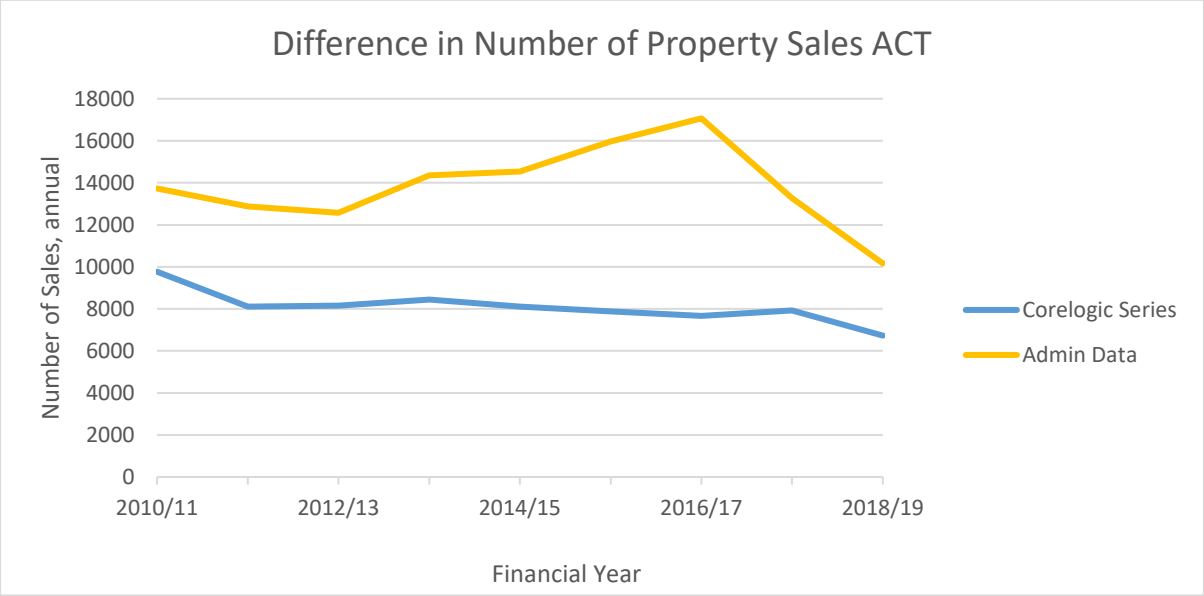
4. Differences in the Corelogic and administrative data on numbers of ACT sales

One concern, which was raised above and in the main report, is differences in the trend of property sale numbers observed between the Corelogic data and ACT's administrative data (see Figure 6). Specifically, the ACT unit record data show an increase in sales during the first five years of the reform. Sales numbers drop sharply from 2017/18, which is due to an administrative change (the introduction of the Barrier Free Model). In cases of off-the-plan and land only transactions, the Barrier Free Model allowed payment of stamp duty to be deferred until completion of construction as opposed to the previous requirement that stamp duty be paid 12 months after the transaction/execution date. This has resulted in some property transactions not yet being included in the current extraction of ACT stamp duty data, since these transactions will only be logged once stamp duty has been paid. Therefore, the count of transactions in 2017-18 and 2018-19 is 'artificially' low in the current data extract. We therefore also estimate models using only the pre-September 2017 data to check whether this administrative change impacts our estimates.

In contrast to the administrative data, our Corelogic data does not show an increase in sales for the years after 2012, but rather a sideways movement over the same period. This could have contributed to our finding that there was a reduction in both the number of property

sales and the overall sales value.[4] To address this concern we also use administrative data as a separate source of information to see if our results are affected in any way. The results from the NSW and ACT administrative data show that the observed differences in the Corelogic data are not driving our findings. These alternative estimates are presented at the end of the appendix.

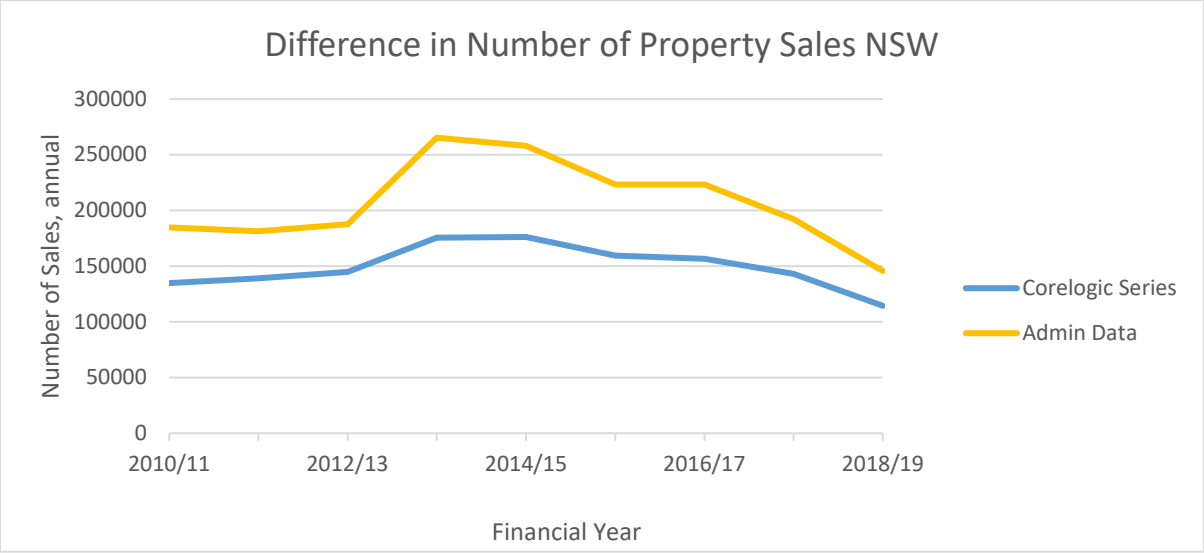
Figure 5 - Difference in number of property sales between the two data sources ACT



Similarly, when we compare the NSW administrative data to the Corelogic data, the overall differences appear to be similar in nature, with the number of salesin the administrative data consistently higher than the Corelogic numbers (see Figure 7). This provides more evidence that the Corelogic adjustments of raw numbers received from the state is consistent across states. These adjustments do not seem to be specific to the ACT. These anomalies are differenced out in our estimation methodology which explains why, despite the large apparent differences in the data, we obtain similar impact estiamtes from the two data sources

[4] The overall sales value is calculated on the basis of number sales times price.

Figure 7 - Difference in number of property sales between the two data sources NSW



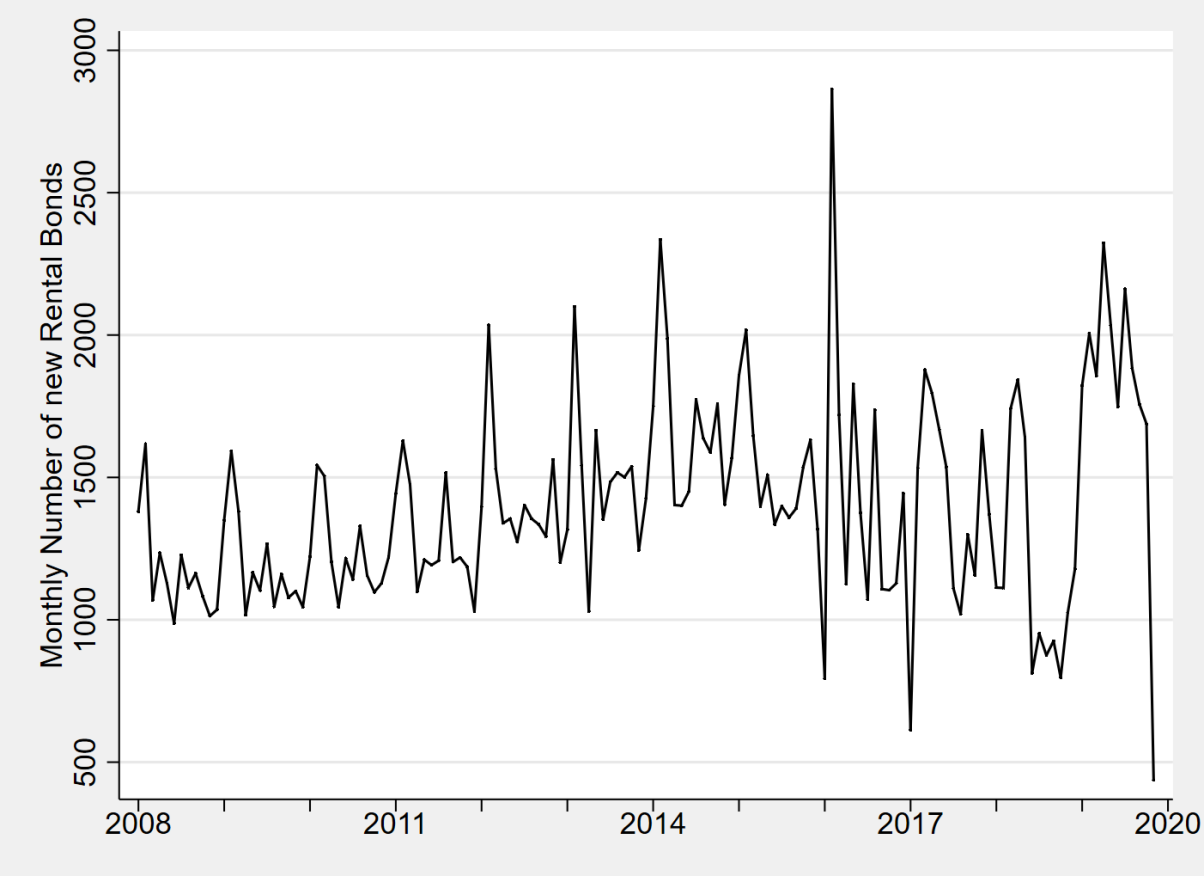
5. The rental bonds data

To answer questions about rental availability we cannot use Corelogic data, as it doesn't provide information on the number of advertised rental properties. Instead, we use administrative data on the number of rental bonds filed in the ACT. In lieu of observing the actual stock of rental properties, observing the flow of rental places is the next best alternative. Moreover, unless the rate of rental contracts concluding changes significantly over our sample period, the flow is a suitable proxy for rental availability in the ACT.

However, while the Corelogic data are available nationally, the ACT rental bond data are only available for ACT suburbs. As a result, we cannot control for common shocks over time as we have done in the difference-in difference approach in the cross-state sample. This presents implications for our results. For example, assume that at the same time as the ACT tax reform was introduced, a national trend emerged shifting preferences away from renting properties and towards buying one's own house or unit (this could happen if interest rates decreased). In the absence of data on the other states and territories, we could wrongly attribute this shift to the tax reform rather than to a change in national preferences.

With these caveats in mind, descriptive data on rental bonds from the ACT are provided below. Figure 5 shows the monthly amount of bonds registered with the ACT government. For each month we observe the number of rental bonds registered. Landlord are required to register a received rental bond, so unless they elect not to take a bond, new rental agreements will be counted in this data. It is common practice to collect a rental bond upon signing the lease.

Figure 6 - Monthly rental bonds registered - ACT time series data 2008 - 2019



We observe a seasonally volatile series which seems to become more volatile in 2012. Eyeballing the effect of the tax reform is difficult with the descriptive statistics as there is no clear break observable in 2012. A regression analysis is required to detect a possible effect. However, the graphs demonstrate the need to control for seasonal variation in the regression. This is done by including month dummies in the regression.

Table 1 below is replicated from Table 19 of the main report. It presents the results of a time series analysis with the number of registered rental bonds as the dependent variable. We use month fixed effects and a linear trend as basic controls for an existing trend and seasonality. In column 1 we only include the reform dummy to measure the effect. The reform dummy is a variable which is one for all months after the start of the reform and zero before. It will capture a general shift in the number of new rentals after the reform. In column 2 and 3 we add a number of extra controls and in column 4 we introduce a reform trend. The reform trend is a trend variable that starts with the introduction of the reform and zero in months prior to the reform. It captures any change in the trend after the reform.

Throughout the specifications, the reform dummy coefficient remains large and significant. The reform's variance inflation factor (VIF) indicates that we do not have to be overly concerned with multicollinearity. The reform coefficient suggests that more than 200 additional rental properties are available on the market each month due to the reform.

Table 1 (Table 19 from main report) - Effect of the tax reform on new rentals – time series analysis

Table 19 - Effect of the tax reform on number of new rentals - time series analysis

VARIABLES	(1) Number of Bonds	(2) Number of Bonds	(3) Number of Bonds	(4) Number of Bonds
Reform Dummy	238.243***	208.141***	225.237***	223.037***
	[81.560]	[78.420]	[60.706]	[63.084]
Reform Trend				2.755
				[10.287]
Linear Trend	0.184	0.771	39.659	41.008
	[1.318]	[1.231]	[31.580]	[29.857]
APS Employment		0.010*	0.023**	0.032
		[0.006]	[0.009]	[0.038]
Income			-1.328	-1.570
			[1.986]	[2.510]
Population			-0.069	-0.074
			[0.052]	[0.047]
Month FX	YES	YES	YES	YES
Observations	143	143	132	132
R-squared	0.366	0.371	0.517	0.517
Reform VIF	3.4	3.7	4.3	4.4

Robust standard errors in brackets *** p<0.01, ** p<0.05, * p<0.1

This type of Before-After (BA) analysis is valid to estimate a treatment effect under certain circumstances. Most importantly that there is no other confounding effect on the outcome over the course of the treatment period. We do not have a control group to identify common changes that affect the outcome in both the control and treatment group in order to control for such confounders. Any such changes, like a downturn in the economy which coincides with the treatment period, will impact our estimate in this time series analysis and lead to an erroneous conclusion about the policy impact.

6. Multicollinearity

Multicollinearity is a problem in some instances. We check for multicollinearity issues in our data with a post-regression analysis of the correlation matrix. This is an alternative the variance inflation factor, which in our case is prohibitive to calculate given the large number of dummy variables in our regressions.

In the correlation matrix we observe a complete set of correlations between pairs of variables. We are only concerned about the correlation with our variable of interest, the reform dummy. Correlation between the other covariates is not biasing our results. The higher the correlation between the reform variable and any of the added controls, the less likely our measure of the reform estimate will be consistent. Correlation values in excess of 0.55 should raise concern about issues of multicollinearity.

In the results section for each regression, we report the largest correlation measure between the reform dummy and any other included controls as a measure of the severity of multicollinearity. Reporting the whole correlation matrix is impractical because of size and the largest correlation value from the correlation matrix gives a good indicator of multicollinearity for each regression.

7. Alternative control groups as counterfactuals

All suburbs in the ACT belong to the treatment group, as they all experienced the tax reform introduced in 2012. All other states and territories are part of the control group since they were unaffected by the ACT's tax reform.

Taking all Australian suburbs as the counterfactual is advantageous since we can utilize all available data. However, it is by no means certain that this control group - the entirety of all suburbs outside of the ACT - is the best control group. We therefore use a number of different subsets of the rest of Australia as alternative control groups in our robustness tests.

First, we reduced the control group to include only Australian States and Territory's capital cities. A second control group was limited to suburbs in NSW & VIC; as the two neighbouring states to the ACT, they may possibly be the most similar property markets. A third control group limited the analysis to suburbs from the capital cities of the neighbouring states (Melbourne and Sydney).

All three additional control groups could prove suitable candidates for a counterfactual to ACT's suburbs. However, each of these sets of counterfactuals has fewer and fewer observations, which tends to reduce the significance of the results. Furthermore, fewer observations can worsen multicollinearity. While the results from all capital city suburbs are presented in the main report, alongside the results for all Australian suburbs, the Results for all of the additional control groups are included at the end of this appendix.

8. Testing the common trend assumption

To verify the first assumption of the difference-indifference method, the common trends assumption, we run tests to see if the pre-treatment trends in the treatment and control groups are similar. In this section we present the results for the common trends tests.

A typical way to test if this assumption holds is to run a regression for the period before the policy implementation. In such regression we include a time trend for the treatment group. If the coefficient on the time trend were to be significant it would mean that there is significant variation between treatment and control group which the added time trend picks up. In this case the common trends assumption fails. If the time trend coefficient is not significantly different from zero, we take this as support for the assumption of a common trend prior to the policy intervention.

This test has the null hypothesis of a common trend between treatment and control groups. Rejecting the null hypothesis would imply that the two groups do not have common trends.

However, failing to reject the parallel trend test in the pre-treatment period is not the same as confirming parallel trends between control and treatment groups.

The following tables present test results of the common trends assumption for the Corelogic data. For each data series; total sales value, property price, property turnover and rental price we have tested the hypothesis for four control groups; all suburbs, only capital city suburbs, suburbs from a subset of states (NSW and Victoria), capital cities suburbs from a subset of states and with controls.

We find that we do not reject the hypothesis of common trends between the ACT and control group for almost all of the tests. When the control group is restricted to only the suburbs of some capital cities, we cannot always reject the null hypothesis.

The following tables present the coefficient for the tested trend for the different control groups (columns) and across the different dependent variables (tables).

Table 2 - Common trend assumption tests - property market sales value

Log(Sales Value)	(1) All Suburbs	(2) All City Suburbs	(3) Subset States Suburbs	(4) Subset City Suburbs	(5) Subset City Suburbs & Controls
Trend Test	0.002 [0.002]	0.003 [0.002]	0.002 [0.002]	0.003 [0.002]	0.005 [0.003]
Observations	136,707	65,467	98,425	43,964	43,964
R-squared	0.792	0.736	0.801	0.750	0.751

Robust standard errors in brackets; ** p<0.01, * p<0.05

Table 3 - Common trend assumption tests - property prices

Log(Property Price)	(1) All Suburbs	(2) All City Suburbs	(3) Subset States Suburbs	(4) Subset City Suburbs	(5) Subset City Suburbs & Controls
Trend Test	0.000 [0.001]	0.000 [0.001]	0.001 [0.001]	0.001* [0.001]	0.002 [0.001]
Observations	136,707	65,467	98,425	34,022	34,022
R-squared	0.776	0.806	0.785	0.816	0.817

Robust standard errors in brackets; ** p<0.01, * p<0.05

Table 4 - Common trend assumption tests - number of sales

Log(Number Sold)	(1) All Suburbs	(2) All City Suburbs	(3) Subset States Suburbs	(4) Subset City Suburbs	(5) Subset City Suburbs & Controls

Trend Test	0.002	0.003	0.002	0.004*	0.004
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Observations	137,638	66,350	99,206	44,701	44,701
R-squared	0.771	0.747	0.783	0.762	0.762

Robust standard errors in brackets; ** p<0.01, * p<0.05

Table 5 - Common trend assumption tests - property prices

Log(Rental Price)	(1) All Suburbs	(2) All City Suburbs	(3) Subset States Suburbs	(4) Subset City Suburbs	(5) Subset City Suburbs & Controls
Trend Test	0.000	0.000	0.001	0.002**	0.000
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Observations	60,608	38,346	45,101	16,187	16,187
R-squared	0.964	0.962	0.964	0.959	0.960

Robust standard errors in brackets; ** p<0.01, * p<0.05

9. Extended results tables

The following pages include the extended result tables of our analysis of the economic and affordability effects of the ACT tax reform. In particular, you will find results for the full set of alternative control groups in the tables below.

Overview

The order of the tables is the same as in the main report. The table numbers are also the same as those in the main report for ease of reference. All tables are exclusive to either house or unit observations. The results tables typically include additional results using NSW and Victoria as the control group in column 4. Column 5 is a combination of reducing the control group to suburbs from capital cities and NSW or Victoria. In other words, suburbs in Sydney and Melbourne are the counterfactual to the ACT in those cases.

All regressions are estimated as log-log specification, as the price series are log-normally distributed. This log-log specification is typical in the property tax literature and has the nice side effect of allowing us to read the coefficients on the continuous variables as elasticities and easily covert coefficients on dummy variables, making the interpretation straight forward.

As indicated in the main report, restricting the control group to smaller subgroups reduces the number of observations, which in turn can decrease the power of the regression and could lead to greater issues of multicollinearity. The tables below show that in almost all cases, the restriction to NSW and Victoria or to just suburbs in Sydney and Melbourne leads to an increase in the error term and to an increase in the correlation of the reform variable with the controls.

As we have confirmed the common trends assumption for all four control groups, it remains a matter of judgement and involves considering the trade-off between choosing all Australian suburbs or a smaller subset of Australian suburbs, but with larger error terms and greater concerns about possible multicollinearity. While we only present the results of the full sample and the alternative of capital cities in the main report, the conclusion in the main reports refers to robustness checks across all four counterfactuals.

Results

The first two tables are present the results on the economic impact on overall value of the house and unit markets.

Table 6 (from main report)- Effect of the tax reform on the housing market - estimations in logs

Log(Sales Volume)	(1) Full Sample	(2) Full Sample	(3) Capital Cities	(4) NSW & VIC	(1) Capital Cities, NSW & VIC
Reform Dummy	-0.103	0.031	-0.055	-0.042	-0.071
	[0.021]**	[0.022]	[0.022]*	[0.025]	[0.025]**
Log(Income)		3.102	3.053	2.311	2.920
		[0.080]**	[0.108]**	[0.288]**	[0.311]**
Log(Population)		3.129	3.961	2.141	3.753
		[0.128]**	[0.184]**	[0.251]**	[0.385]**
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	543,443	515,524	234,695	261,777	120,366
Number of Suburbs	10,116	10,045	2,806	5,069	1,455
Adj. R-squared	0.78	0.79	0.74	0.80	0.75
Max in Cor. Matrix	.	0.25	0.26	0.59	0.61

* p<0.05; ** p<0.01

Table 7b (not in main report) - Effect of the tax reform on the unit market - estimations in logs

Log(Sales Volume)	(1) Full Sample	(2) Full Sample	(3) Capital Cities	(4) NSW & VIC	(1) Capital Cities, NSW & VIC
Reform Dummy	-0.396	-0.222	-0.274	-0.284	-0.297
	[0.028]**	[0.031]**	[0.031]**	[0.041]**	[0.042]**
Log(Income)		3.414	3.351	2.788	3.457
		[0.160]**	[0.185]**	[0.402]**	[0.418]**
Log(Population)		2.479	3.216	0.835	2.495
		[0.222]**	[0.276]**	[0.406]*	[0.505]**
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	219,559	208,943	137,577	118,309	81,139
Number of Suburbs	3,799	3,781	2,112	1,980	1,135
Adj. R-squared	0.76	0.77	0.77	0.80	0.78
Max in Cor. Matrix	.	0.34	0.35	0.70	0.70

* p<0.05; ** p<0.01

The next two tables present the results on trends in the overall value of the house and unit markets. They are a one to one replication from the main report, for completion to have all results in one place.

Table 8 (from main report)- Trend in the effect of the tax reform on the housing market - estimations in logs

Log(Sales Volume)	(1) Reform Trend	(2) Trend & Controls	(3) Trend & Capital Cities	(4) Sample till 2015	(5) Sample till 2017	(6) Full Sample
Reform Dummy	-0.125	-0.013	-0.078	0.013	0.019	0.031
	[0.025]**	[0.025]	[0.026]**	[0.020]	[0.021]	[0.022]
Reform Trend	0.005	0.010*	0.005			
	[0.006]	[0.006]	[0.006]			
Log(Income)		3.108	3.059	2.606	2.928	3.102
		[0.080]**	[0.108]**	[0.081]**	[0.080]**	[0.080]**
Log(Population)		3.115	3.945	1.022	2.878	3.129
		[0.129]**	[0.185]**	[0.174]**	[0.140]**	[0.128]**
Time & Suburb FE	YES	YES	YES	YES	YES	YES
Observations	543,443	515,524	234,695	339,387	457,063	515,524
Number of Suburbs	10,116	10,045	2,806	9,343	9,848	10,045
Adj. R-squared	0.78	0.79	0.74	0.79	0.79	0.79
Max in Cor. Matrix	0.64	0.63	0.63	0.41	0.26	0.25

* p<0.05; ** p<0.01

Table 9 (from main report) - Trend in the effect of the tax reform on the unit market - estimations in logs

Log(Sales Volume)	(1) Reform Trend	(2) Trend & Controls	(3) Trend & Capital Cities	(4) Sample till 2015	(5) Sample till 2017	(6) Full Sample
Reform Dummy	-0.333	-0.174	-0.245	-0.269	-0.270	-0.222
	[0.049]**	[0.050]**	[0.050]**	[0.036]**	[0.031]**	[0.031]**
Reform Trend	-0.014	-0.012	-0.007			
	[0.008]	[0.009]	[0.009]			
Log(Income)		3.399	3.337	2.369	3.181	3.414
		[0.161]**	[0.187]**	[0.171]**	[0.161]**	[0.160]**
Log(Population)		2.504	3.238	2.252	3.673	2.479
		[0.223]**	[0.278]**	[0.343]**	[0.258]**	[0.222]**
Time & Suburb FE	YES	YES	YES	YES	YES	.
Observations	219,559	208,943	137,577	139,698	186,907	208,943
Number of Suburbs	3,799	3,781	2,112	3,601	3,740	3,781
Adj. R-squared	0.76	0.77	0.77	0.77	0.77	0.77
Max in Cor. Matrix	0.81	0.78	0.77	0.41	0.33	0.34

* p<0.05; ** p<0.01

The next two tables present the results on the reform effect on prices in the house and unit markets.

Table 10 (from main report) - Effect of the tax reform on house prices - estimations in logs

Log(House Price)	(1) Full Sample	(2) Full Sample	(3) Capital Cities	(4) NSW & VIC	(5) Capital Cities, NSW & VIC
Reform Dummy	-0.008	0.094	0.021	0.045	0.048
	[0.013]	[0.013]**	[0.013]	[0.013]**	[0.013]**
Log(Population)		1.910	2.568	-0.885	-1.724
		[0.067]**	[0.086]**	[0.144]**	[0.141]**
Log(Income)		2.267	2.324	1.774	2.800
		[0.050]**	[0.047]**	[0.161]**	[0.166]**
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	543,443	515,524	234,695	261,777	120,366
Number of Suburbs	10,116	10,045	2,806	5,069	1,455
Adj. R-squared	0.75	0.76	0.81	0.77	0.81
Max in Cor. Matrix	.	0.21	0.11	0.46	0.47

* $p<0.05$; ** $p<0.01$

Table 11b (not in main report) - Effect of the tax reform on unit prices - estimations in logs

	(1) Full Sample	(2) Full Sample	(3) Capital Cities	(4) NSW & VIC	(5) Capital Cities, NSW & VIC
Reform Dummy	-0.110	0.044	0.002	0.001	0.017
	[0.008]**	[0.010]**	[0.009]	[0.011]	[0.011]
Log(Population)		0.965	1.041	-3.124	-3.960
		[0.112]**	[0.143]**	[0.180]**	[0.189]**
Log(Income)		2.608	2.508	2.175	2.808
		[0.092]**	[0.076]**	[0.118]**	[0.119]**
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	219,559	208,943	137,577	118,309	81,139
Number of Suburbs	3,799	3,781	2,112	1,980	1,135
Adj. R-squared	0.67	0.68	0.69	0.74	0.70
Max in Cor. Matrix	.	0.19	0.44	0.69	0.69

* $p<0.05$; ** $p<0.01$

As the introduction of the Barrier Free Model in 2017 led to deferred recordings of sales for off-the-plan and land only purchases, this engendered an artificial overall reduction in recorded sales numbers. As a result, we might be concerned about the last two years of our sample driving the results, in particular, for the unit market. While the CoreLogic data does not seem to exhibit the reduction in property sales in 2017-18 and 2018-19 years (see comparison above), we have checked that our results are not sensitive to excluding those two years by re-estimating the model for a subsample of the data excluding the last two years. The results are reported in Table 14b and are nearly identical to the full sample results.

Table 14c (not in main report) - Effect of the tax reform on unit prices- estimations in logs 2009-2017

Log(Number Sold)	(1) All Suburbs	(2) All Suburbs	(3) Capital Cities	(4) NSW & VIC	(5) Capital Cities, NSW & VIC
Reform Dummy	-0.092	0.051	0.021	0.012	0.026
	[0.008]**	[0.010]**	[0.009]*	[0.010]	[0.010]**
Log(Population)		0.015	-0.013	-4.004	-5.475
		[0.117]	[0.151]	[0.207]**	[0.205]**
Log(Income)		2.260	2.280	1.840	2.391
		[0.085]**	[0.074]**	[0.116]**	[0.115]**
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	175,288	175,288	115,643	98,872	68,080
Number of Suburbs	3,707	3,707	2,086	1,948	1,121
Adj. R-squared	0.67	0.68	0.69	0.74	0.71
Max Correlation Matrix	.	0.25	0.43	0.62	0.61

* $p<0.05$; ** $p<0.01$

The next two tables present the results on the effects on the number of sales in the house and unit markets.

Table 12 (from main report) - Effect of the tax reform on the number of house sales - estimations in logs

Log(Number Sold)	(1) Full Sample	(2) Full Sample	(3) Capital Cities	(4) NSW & VIC	(5) Capital Cities, NSW & VIC
Reform Dummy	-0.096	-0.062	-0.075	-0.084	-0.117
	[0.029]**	[0.028]*	[0.028]**	[0.023]**	[0.023]**
Log(Population)		1.225	1.414	3.000	5.217
		[0.108]**	[0.168]**	[0.245]**	[0.376]**
Log(Income)		0.854	0.769	0.598	0.215
		[0.069]**	[0.105]**	[0.298]*	[0.328]
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	550,554	522,180	241,061	267,468	125,920
Number of Suburbs	10,117	10,046	2,806	5,070	1,455
Adj. R-squared	0.76	0.77	0.74	0.78	0.76
Max in Cor. Matrix	.	0.11	0.15	0.24	0.30

* $p<0.05$; ** $p<0.01$

Table 13b (not in main report) - Effect of the tax reform on the number of unit sales - estimations in logs

Log(Number Sold)	(1) Full Sample	(2) Full Sample	(3) Capital Cities	(4) NSW & VIC	(5) Capital Cities, NSW & VIC
Reform Dummy	-0.285	-0.265	-0.275	-0.284	-0.313
	[0.027]**	[0.029]**	[0.030]**	[0.040]**	[0.041]**
Log(Population)		1.516	2.174	3.963	6.429
		[0.185]**	[0.238]**	[0.377]**	[0.473]**
Log(Income)		0.810	0.848	0.629	0.665
		[0.138]**	[0.172]**	[0.385]	[0.405]
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	220,220	209,572	138,177	118,877	81,686
Number of Suburbs	3,799	3,781	2,112	1,980	1,135
Adj. R-squared	0.74	0.74	0.75	0.76	0.77
Max in Cor. Matrix	.	0.29	0.33	0.71	0.71

* $p<0.05$; ** $p<0.01$

As the introduction of the Barrier Free Model in 2017 led to deferred recordings of sales for off-the-plan and land only purchases, this engendered an artificial overall reduction in recorded sales numbers. As a result, we might be concerned about the last two years of our sample driving the results, in particular, for the unit market. The specification is rerun in Table 15b (not in the main report) for a subsample of the data excluding the last two years of our sample. We find an even larger decrease for this subsample.

Table 15c (not in the main report) - Effect of the tax reform on number of unit sales - estimations in logs 2009-2017

Log(Number Sold)	(1) All Suburbs	(2) All Suburbs	(3) Capital Cities	(4) NSW & VIC	(5) Capital Cities, NSW & VIC
Reform Dummy	-0.329	-0.397	-0.440	-0.276	-0.323
	[0.029]**	[0.031]**	[0.032]**	[0.040]**	[0.042]**
Log(Population)		3.347	4.622	6.069	9.080
		[0.233]**	[0.297]**	[0.527]**	[0.646]**
Log(Income)		0.558	0.565	1.364	1.406
		[0.140]**	[0.171]**	[0.371]**	[0.392]**
Time & Suburb FE	.	.	.	.	.
Observations	175,724	175,724	116,061	99,258	68,452
Number of Suburbs	3,707	3,707	2,086	1,948	1,121
Adj. R-squared	0.74	0.74	0.75	0.77	0.77
Max in Cor. Matrix	.	.	.	.	.

* $p<0.05$; ** $p<0.01$

The next two tables present the results of the rental prices in the house and unit rental markets.

Table 14 (Columns (1)-(3) from main report; Columns (4) – (5) not in main report) - Tax reform effect on house rental prices - estimations in logs

Log(Median Rent)	(1) Full Sample	(2) Full Sample	(3) Capital Cities	(4) NSW & VIC	(5) Capital Cities, NSW & VIC
Reform Dummy	-0.077	-0.014	-0.026	-0.008	0.008
	[0.006]**	[0.008]	[0.007]**	[0.008]	[0.008]
Log(Income)		1.505	1.471	1.419	1.679
		[0.057]**	[0.029]**	[0.057]**	[0.054]**
Log(Population)		1.121	1.344	-0.071	0.064
		[0.061]**	[0.070]**	[0.082]	[0.088]
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	263,249	248,629	153,750	122,691	78,559
Number of Suburbs	3,435	3,420	1,988	1,734	1,032
Adj. R-squared	0.91	0.92	0.95	0.96	0.96
Max in Cor. Matrix	.	0.52	0.23	0.45	0.40

* p<0.05; ** p<0.01

Table 15b (not in main report) - Tax reform effect on unit rental prices - estimations in logs

Log(Median Rent)	(1) Full Sample	(2) Full Sample	(3) Capital Cities	(4) NSW & VIC	(5) Capital Cities, NSW & VIC
Reform Dummy	-0.166	-0.082	-0.084	-0.069	-0.054
	[0.011]**	[0.013]**	[0.012]**	[0.011]**	[0.011]**
Log(Income)		1.516	1.464	1.499	1.655
		[0.074]**	[0.048]**	[0.077]**	[0.074]**
Log(Population)		1.231	1.227	-0.187	-0.230
		[0.099]**	[0.110]**	[0.105]	[0.112]*
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	134,234	126,901	90,643	76,135	57,672
Number of Suburbs	2,012	1,998	1,297	1,086	733
Adj. R-squared	0.90	0.91	0.93	0.95	0.94
Max in Cor. Matrix	.	0.41	0.26	0.23	0.18

* p<0.05; ** p<0.01

The following two tables presents the results for a quintile analysis of the effects on rental prices for both house and unit rental markets. They are a one to one replication from the main report, for completion to have all results in one place.

Table 16 (from main report) - Tax reform effect on house rental prices - quantile estimations in logs

Log(Median Rent)	(1) 1st Quintile	(2) 2nd Quintile	(3) 3rd Quintile	(4) 4th Quintile	(5) 5th Quintile
Reform Dummy	-0.060	-0.011	-0.039	-0.065	0.087
	[0.014]**	[0.013]	[0.010]**	[0.023]**	[0.021]**
Log(Income)	0.680	1.202	1.308	1.328	2.841
	[0.085]**	[0.061]**	[0.064]**	[0.073]**	[0.214]**
Log(Population)	0.782	0.855	1.011	1.365	1.022
	[0.147]**	[0.115]**	[0.118]**	[0.151]**	[0.161]**
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	40,308	42,221	42,534	41,947	45,438
Number of Suburbs	438	434	445	441	499
Adj. R-squared	0.82	0.81	0.82	0.87	0.87

* $p<0.05$; ** $p<0.01$

Table 17 (from main report)- Tax reform effect on unit rental prices - quintile estimations in logs

Log(Median Rent)	(1) 1st Quintile	(2) 2nd Quintile	(3) 3rd Quintile	(4) 4th Quintile	(5) 5th Quintile
Reform Dummy	-0.130	-0.119	-0.118	-0.047	0.026
	[0.037]**	[0.023]**	[0.017]**	[0.018]**	[0.017]
Log(Income)	1.048	1.118	1.423	1.405	2.242
	[0.140]**	[0.105]**	[0.128]**	[0.126]**	[0.205]**
Log(Population)	1.289	1.222	0.741	0.766	1.117
	[0.237]**	[0.233]**	[0.236]**	[0.218]**	[0.241]**
Time & Suburb FE	YES	YES	YES	YES	YES
Observations	18,335	21,124	19,485	20,262	21,355
Number of Suburbs	216	228	203	212	218
Adj. R-squared	0.85	0.80	0.86	0.89	0.85

* $p<0.05$; ** $p<0.01$

As an additional control variable, we merge Corelogic data with data from the Australian Public Service Commission on APS employment statistics and use it, in addition to the population and disposable income data, as an additional control variable. The results are qualitatively similar to those presented in the main report. In some cases, the inclusion of the APS employment data raises the likelihood of multicollinearity and doesn't add much explanatory power. Therefore, the APS employment variable is not included in the main result tables.

10. NSW and ACT admin data result tables

In this section we present results using administrative data of sales from the ACT and NSW. NSW data is a likely candidate for an appropriate counterfactual given the proximity to the ACT. We re-start table numbering at 20 to avoid any confusion with tables presented above.

The regression approach is identical in this section. The table builds up including more and more controls from the left to the right. We observe that the results vary substantially in magnitude and significance depending on which control variables are included. We thus strongly encourage readers to interpret and apply these results cautiously. The results for houses and unites are shown in separate tables.

Generally, the fourth column in the tables below would be our preferred specification as they control for the maximum number of elements. Controlling for time- and postcode-specific effects should reduce bias in the impact estimate. When conducting difference-in-difference analysis, it is general practice to present results with no control variables and then progressively add control variables as we have done. One reason for this is to see whether or not the additional controls affect the impact estimate. If the estimate changes dramatically from the case of no control variables to a full set of control variables, this is generally viewed as evidence that confounding factors are having a strong impact on the estimate of the policy. This generally leads researchers to have less faith in the estimate, as if observable factors are impacting the estimate, then it is likely that unobservables, which can not be controlled for, are also having a large impact on the estimate. This would mean that even the 'best' estimate with a full set of observable control variables may still be biased.

Note that all the caveats about having an appropriately specified control group still hold.

Table 20 - Tax reform effect on housing market: Difference-in-difference estimates using 2004/05 – 2018/19 administrative data

	(1) Log(Sales Value) b/se	(2) Log(Sales Value) b/se	(3) Log(Sales Value) b/se	(4) Log(Sales Value) b/se
ACT tax reform	-0.233** (0.045)	-0.058 (0.072)	-0.374** (0.050)	-0.201** (0.048)
Time Fixed Effect	Yes	Yes	Yes	Yes
Postcode Fixed Effects	Yes	Yes	Yes	Yes
Log(Income)	No	Yes	No	Yes
Log(Population)	No	No	Yes	Yes
adj.R-squared	0.751	0.752	0.751	0.752
Observations	106546	103103	106546	103103
Postcodes	651	651	651	651

Note: Standard error clusters at state-postcode level * p<0.05, ** p<0.01

Table 21 - Tax reform effect on unit market: Difference-in-difference estimates using 2004/05 – 2018/19 administrative data

	(1) Log(Sales Value) b/se	(2) Log(Sales Value) b/se	(3) Log(Sales Value) b/se	(4) Log(Sales Value) b/se
ACT tax reform	0.100	0.099	-0.176	-0.185
	(0.115)	(0.128)	(0.159)	(0.166)
Time Fixed Effect	Yes	Yes	Yes	Yes
Postcode Fixed Effects	Yes	Yes	Yes	Yes
Log(Income)	No	Yes	No	Yes
Log(Population)	No	No	Yes	Yes
adj.R-squared	0.818	0.819	0.819	0.820
Observations	64811	62692	64811	62692
Postcodes	494	493	494	493

Note: Standard error clusters at state-postcode level * p<0.05, ** p<0.01

Table 22 - Tax reform effect on house prices: Difference-in-difference estimates using 2004/05 – 2018/19 administrative data

	(1) Log(Sales Price) b/se	(2) Log(Sales Price) b/se	(3) Log(Sales Price) b/se	(4) Log(Sales Price) b/se
ACT tax reform	-0.112**	0.036	-0.149**	-0.008
	(0.029)	(0.040)	(0.037)	(0.043)
Time Fixed Effect	Yes	Yes	Yes	Yes
Postcode Fixed Effects	Yes	Yes	Yes	Yes
Log(Income)	No	Yes	No	Yes
Log(Population)	No	No	Yes	Yes
adj.R-squared	0.594	0.593	0.594	0.593
Observations	106546	103103	106546	103103
Postcodes	651	651	651	651

Note: Standard error clusters at state-postcode level * p<0.05, ** p<0.01

Table 23 - Tax reform effect on unit prices: Difference-in-difference estimates using 2004/05 – 2018/19 administrative data

	(1) Log(Sales Price) b/se	(2) Log(Sales Price) b/se	(3) Log(Sales Price) b/se	(4) Log(Sales Price) b/se
ACT tax reform	-0.127*	0.046	-0.071	0.086
	(0.057)	(0.044)	(0.126)	(0.110)
Time Fixed Effect	Yes	Yes	Yes	Yes
Postcode Fixed Effects	Yes	Yes	Yes	Yes
Log(Income)	No	Yes	No	Yes
Log(Population)	No	No	Yes	Yes
adj.R-squared	0.663	0.662	0.663	0.662
Observations	64811	62692	64811	62692
Postcodes	494	493	494	493

Note: Standard error clusters at state-postcode level * p<0.05, ** p<0.01

Table 24 - Tax reform effect on number of house sales: Difference-in-difference estimates using 2004/05 – 2018/19 administrative data

	(1) Log(Sales Numbers) b/se	(2) Log(Sales Numbers) b/se	(3) Log(Sales Numbers) b/se	(4) Log(Sales Numbers) b/se
ACT tax reform	-0.121**	-0.094	-0.226**	-0.193**
	(0.043)	(0.056)	(0.071)	(0.047)
Time Fixed Effect	Yes	Yes	Yes	Yes
Postcode Fixed Effects	Yes	Yes	Yes	Yes
Log(Income)	No	Yes	No	Yes
Log(Population)	No	No	Yes	Yes
adj.R-squared	0.835	0.836	0.835	0.836
Observations	106556	103112	106556	103112
Postcodes	651	651	651	651

Note: Standard error clusters at state-postcode level * p<0.05, ** p<0.01

Table 25 - Tax reform effect on number of unit sales: Difference-in-difference estimates using 2004/05 – 2018/19 administrative data

	(1) Log(Sales Numbers) b/se	(2) Log(Sales Numbers) b/se	(3) Log(Sales Numbers) b/se	(4) Log(Sales Numbers) b/se
ACT tax reform	0.226*	0.052	-0.104	-0.271**
	(0.101)	(0.113)	(0.082)	(0.101)
Time Fixed Effect	Yes	Yes	Yes	Yes
Postcode Fixed Effects	Yes	Yes	Yes	Yes
Log(Income)	No	Yes	No	Yes
Log(Population)	No	No	Yes	Yes
adj.R-squared	0.814	0.815	0.814	0.815
Observations	64814	62693	64814	62693
Postcodes	494	493	494	493

Note: Standard error clusters at state-postcode level * p<0.05, ** p<0.01

As discussed above, due to the introduction of the Barrier Free Model in 2017, we might be concerned about the last two years of our sample driving the results, in particular, for the unit market. Similar to the regression above for the CoreLogic data we now check that our results are not sensitive to excluding those years by re-estimating the model using a subsample of the data excluding the last two years. The results are reported in in the tables below (not in the main report) and are qualitatively and quantitatively similar to the full sample results.

Table 26 - Tax reform effect on house prices: Difference-in-difference estimates using 2004/05 – 2016/17 administrative data

	(1) Log(Sales Price) b/se	(2) Log(Sales Price) b/se	(3) Log(Sales Price) b/se	(4) Log(Sales Price) b/se
ACT tax reform	-0.077*	0.064	-0.184**	-0.048
	(0.034)	(0.041)	(0.038)	(0.043)
Time Fixed Effect	Yes	Yes	Yes	Yes
Postcode Fixed Effects	Yes	Yes	Yes	Yes
Log(Income)	No	Yes	No	Yes
Log(Population)	No	No	Yes	Yes
adj.R-squared	0.596	0.586	0.586	0.586
Observations	148452	92408	92408	92408
Postcodes	1143	651	651	651

Note: Standard error clusters at state-postcode level * p<0.05, ** p<0.01

Table 27 - Tax reform effect on unit prices: Difference-in-difference estimates using 2004/05 – 2016/17 administrative data

	(1) Log(Sales Price) b/se	(2) Log(Sales Price) b/se	(3) Log(Sales Price) b/se	(4) Log(Sales Price) b/se
ACT tax reform	-0.077*	0.075	-0.095	0.062
	(0.034)	(0.046)	(0.136)	(0.121)
Time Fixed Effect	Yes	Yes	Yes	Yes
Postcode Fixed Effects	Yes	Yes	Yes	Yes
Log(Income)	No	Yes	No	Yes
Log(Population)	No	No	Yes	Yes
adj.R-squared	0.596	0.644	0.643	0.644
Observations	148452	56044	56044	56044
Postcodes	1143	492	492	492

Note: Standard error clusters at state-postcode level * p<0.05, ** p<0.01

Table 28 - Tax reform effect on house sales: Difference-in-difference estimates using 2004/05 – 2016/17 administrative data

	(1) Log(Sales Numbers) b/se	(2) Log(Sales Numbers) b/se	(3) Log(Sales Numbers) b/se	(4) Log(Sales Numbers) b/se
ACT tax reform	0.026	-0.114	-0.210*	-0.175**
	(0.067)	(0.058)	(0.082)	(0.054)
Time Fixed Effect	Yes	Yes	Yes	Yes
Postcode Fixed Effects	Yes	Yes	Yes	Yes
Log(Income)	No	Yes	No	Yes
Log(Population)	No	No	Yes	Yes
adj.R-squared	0.830	0.838	0.838	0.838
Observations	148452	92408	92408	92408
Postcodes	1143	651	651	651

Note: Standard error clusters at state-postcode level * p<0.05, ** p<0.01

Table 29 - Tax reform effect on unit sales: Difference-in-difference estimates using 2004/05 – 2016/17 administrative data

	(1) Log(Sales Numbers) b/se	(2) Log(Sales Numbers) b/se	(3) Log(Sales Numbers) b/se	(4) Log(Sales Numbers) b/se
ACT tax reform	0.026	0.025	-0.103	-0.263**
	(0.067)	(0.122)	(0.080)	(0.097)
Time Fixed Effect	Yes	Yes	Yes	Yes
Postcode Fixed Effects	Yes	Yes	Yes	Yes
Log(Income)	No	Yes	No	Yes
Log(Population)	No	No	Yes	Yes
adj.R-squared	0.830	0.817	0.817	0.817
Observations	148452	56044	56044	56044
Postcodes	1143	492	492	492

Note: Standard error clusters at state-postcode level * p<0.05, ** p<0.01