The “Flock” Phenomenon of the Sydney Lockout Laws: Dual Effects on Rental Prices

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THIS VERSION: August 11, 2016

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Abstract

Geographically targeted crime control is a controversial attempt to alleviate crime by targeting “hot spots”, which risks the potential displacement of crime into bordering areas. The 2014 Sydney lockout laws have severely decreased the nightlife economy in the once bustling entertainment district of the CBD, and there have been reports of increased violence in displacement, or “flock”, areas. These laws have also displaced attractive nightlife entertainment hubs into neighbouring suburbs, which may contribute to the land value of the displacement areas. To address the paucity of empirical evidence for the displacement effect of geographical alcohol regulations, this paper investigates the effect of the Sydney lockout laws on rental prices in the displacement areas. We find differential “flock effects”: a negative effect on small dwellings and a positive effect on large dwellings. The former effect is relatively weak and short-lived, while the latter is persistent, indicating that the positive effect dominates in the long run. We speculate that the differential effect arises because of difference in the locations of small and large dwellings. Our results suggest that well-designed geographically targeted alcohol control can enhance social welfare not only in targeted areas but also in surrounding areas.

JEL Codes: K32; R2; R3

Keywords: Alcohol law; Geographically targeted crime control; Displacement; Housing markets; Difference-in-difference, Sydney

1. Introduction

Geographically targeted crime control, which targets "hot spots" rather than the root cause of the crime phenomenon, is a controversial attempt to alleviate the incidence of criminal behaviour. Controversy exists over the concern that targeting the location of offences may lead to the displacement of crime into areas that are exempt from regulations, creating socio-spatial ghettos in bordering areas (Fischer et al., 2004; Bowers et al., 2011; Telep et al., 2014). The Sydney lockout laws, which were implemented in 2014 as part of the New South Wales Government's crackdown on drug and alcohol-fuelled violence, have restricted popular and iconic establishments in the once bustling Sydney Entertainment District (SED) (Spicer, 2015), and have diverted late night partygoers to alternative destinations that have bars, clubs, and other licensed venues. These displacement, or "flock", areas are now flooded with those same revellers who have the potential to commit violence in the SED (Ralston, 2015), and there have been reports of increased violence and feelings of unsafety, including the death of a man in Waterloo as the result of a coward punch (Levy, 2015). A recent article by Donnelly et al. (2016) reports an increase in assaults at The Star casino, which is outside the SED, since the introduction of the lockout laws.

While evidence tends to show that the decrease in crime in targeted areas is much larger than the increase in crime in flock areas, implying the overall positive effect of geographically targeted policies (Guerette and Bowers, 2009; Bowers et al., 2011; Johnson et al., 2012; Telep et al., 2014; Donnelly et al., 2016), the potentially negative impact on flock area communities inevitably leads to intense public debate. At the same time, the Sydney lockout laws have also led to the displacement of nightlife entertainment (Spicer, 2015). The new function of flock areas as "trendy" entertainment

hubs may be an attractive feature for certain individuals and may have contributed to local housing demand as a result of the proximity to nightlife or increased job opportunities. The hasty passing of the lockout laws did not consider the possible unintended effect of the laws on surrounding areas, and despite a flood of media articles and concerns expressed by residents, there is little empirical evidence for the effect of the lockout laws on the displacement areas. Such evidence is critical in the cost-benefit consideration of geographically targeted crime control policies.

In this paper, we study the causal effect of the "flock" phenomenon of the Sydney lockout laws on rental prices in the local housing market. To separate the causal effect of the lockout laws from secular housing market trends, we rely on a quasi-experimental research design in which we apply a difference-in-difference (DID) approach to the postcode-level weekly rent data. Given our concern about potential heterogeneity in the causal effect, we also conduct sub-sample analysis to separately evaluate the causal effect by dwelling size.

A variety of methods are used in the literature to quantify the indirect and intangible cost of crime, such as hedonic regression (e.g. Tita, Petras, and Greenbaum, 2006) and the life satisfaction approach (e.g. Manning, Fleming, and Ambrey, 2015). In this paper, we follow the spirit of hedonic regression and assume that housing market prices provide sufficient statistics to measure the value of a local neighbourhood, because housing prices are determined by a market mechanism that reflects the neighbourhood's various short-term and long-term factors consumers take into consideration. There are myriad studies on the negative effect of crime on local housing demand (Rizzo, 1979; Dubin and Goodman, 1982; Cullen and Levitt, 1999; Tita, Petras, and Greenbaum, 2006; Linden and Rockoff, 2008; Ihlanfeldt and Mayock, 2010;

Klimova and Lee, 2014). Conversely, the Allen Consulting Group (2012) reports a positive relationship between the density of licensed premises and housing rental prices in New South Wales, which suggests that renters are attracted to areas with a high density of licensed premises. Bianchi (2015) also suggests that the culture of late-night drinking in entertainment venues may be a drawcard for young renters at the same time as causing increased security concerns for families. The standard hedonic approach may yield misleading estimates as a result of various confounding factors, but we employ a rigorous causal framework in a similar manner to other recent studies (Linden and Rockoff, 2008; Klimova and Lee, 2014). We study rental prices rather than housing prices, which is motivated by the following two facts: rental price data offer a substantially larger number of observations than housing price data, and rental prices react more quickly to exogenous changes than housing prices. The use of rental prices is therefore suitable for evaluating not only the long-term effect of lockout laws but also the short-term effect.

Our results are summarised as follows. The overall-effect models show that the introduction of the lockout laws has had no statistically significant causal effect on median rental prices. However, sub-sample analysis reveals differential causal effects: a negative effect on one-bedroom dwellings and a positive effect on 3+ bedroom dwellings. The former effect is relatively weak and short-lived, while the latter is persistent, indicating that the positive effect dominates in the long run. These opposite effects offset each other and consequently result in the insignificant estimate in the overall-effect models. We speculate that the differential effect arises because of spatial heterogeneity: smaller dwellings tend to be located closer to main bars and entertainment strips whereas larger dwellings tend to be in a quieter neighbourhood of

the postcode. The dual effect of the lockout laws suggests that well-designed geographically targeted alcohol control can be a cost-effective approach even when its effect on the displacement areas is taken into consideration.

2. The Sydney Lockout Laws

On 21 January 2014 the New South Wales State Government announced new restrictions on licensed premises to reduce alcohol-related violence. The legislation, which took effect on 24 February 2014, included a lockout of new patrons to hotels, registered clubs, nightclubs, and karaoke bars [1] after 1:30 am in the Sydney Entertainment District (SED), which comprises areas of Sydney Central Business District (CBD), Woolloomooloo, Potts Point, Kings Cross, and parts of Darlinghurst, including Oxford Street; cessation of alcohol service in these venues at 3:00 am; the banning of designated "troublemakers" from entering these venues; and a ban on takeaway alcohol sales after 10:00 pm across New South Wales (NSW Government, 2014; Donnelly et al., 2016). Given the prevalence of late-night drinking and pre-drinking culture in Australia (Miller et al., 2016), limiting late-night access to licensed venues has potential for significantly reducing alcohol-fuelled violence and crime.

Studies and reports offer clear evidence that the Sydney lockout laws have reduced the amount of alcohol related violence in the SED (Fulde et al., 2015**;** Menéndez et al., 2015; Ralston, 2015; Donnelly et al., 2016). Fulde et al. (2015) report a significant reduction in alcohol-related injuries and trauma presentations at a nearby hospital. Menéndez et al. (2015) also find that the Sydney lockout laws have reduced the incidence of assault in the Kings Cross and CBD entertainment precincts.

[1] Small bars (maximum 60 people), most restaurants, and tourism accommodation establishments are exempt. Venues currently licensed to stay open after 3:00 am can do so without alcohol service (NSW Government, 2014)

3. Past Studies on the Effect of Nightlife Restriction Laws

Violence in entertainment districts is a major problem across urban landscapes around the world, and geographically targeted crime control has been widely used (Braga et al., 2014). There is resounding evidence both in Australia and the world that government enforced restrictions on alcohol access reduce crime and violence. In Australia, lockout laws were implemented in the Newcastle CBD in March 2008, and Kypri et al. (2011) report a dramatic 37% decrease in assaults in the restricted area. Douglas (1998) studies the restriction of access to alcohol in the community of Halls Creek in Western Australia, and finds that a reduction in the trading hours of licenced premises is associated with a reduction in the consumption of alcohol, the incidence of crime, alcohol-related presentations at hospital, and the incidence of domestic violence. A significant reduction in the number of violent incidents due to lockout legislation is also reported for Queensland (Mazerolle et al., 2012). Internationally, Voas et al. (2002) report favourable consequences of the early closure of bars around the US-Mexico border. In Amsterdam, the reverse was shown to occur when extended opening hours correlated with a significant increase in alcohol-related injuries and violence (De Goeij et al., 2015).

The evidence for the effect of lockout laws on displacement areas is mixed and relatively scarce. Although most studies are observational rather than based on a rigorous quasi-experimental design, a series of reviews have found that the displacement of crime, if any, is uncommon and small (Guerette and Bowers, 2009; Bowers et al., 2011; Johnson et al., 2012; Telep et al., 2014). In Australia, Kypri et al. (2011) report that there has been no overall geographic displacement of assault from the

Newcastle CBD to a nearby area with a similar night-time economy. Mazerolle et al. (2011) also find no displacement of violence to surrounding areas following lockout legislation in Queensland. In the case of the Sydney lockout laws, however, the geographical displacement of violence and crime has been reported as discussed above (Levy, 2015; Donnelly et al. 2016), whereas Menéndez et al. (2015) do not find statistically significant changes in displacement areas.

4. Data

4.1. Data Sources and Sample Selection

The main dataset is drawn from the Rent and Sales Reports, a quarterly report of weekly rents in the greater metropolitan region of Sydney published by the NSW Government's Housing department.[2] Using the supplementary table 'A1: Median Weekly Rents - Greater Metropolitan Region by Postcodes - All Dwellings' (NSW Government: Family & Community Services, 2015), we compile quarterly postcode-level panel data that provide the median weekly rental price by number of bedrooms – 1, 2, 3, and 4+ bedrooms. In the report, the median rental price is left blank if less than ten active bonds are registered in the postcode, but missing values are rare due to our focus on inner-city areas, where a large number of rental properties are always on the market.

The main analysis relies on data from the June 2013, September 2013, December 2013, June 2014, September 2014, December 2014, March 2015, June 2015, September 2015, and December 2015 tables (NSW Government: Family & Community Services, 2015, Issues 104, 105, 106, 108, 109, 110, 111, 112, 113, and 114, respectively). The March 2014 dataset is excluded from the analysis because it relates to

[2] The data are publicly accessible from the 'Reports, Plans & Paper' section at the website of Housing NSW, the NSW Government's housing department, www.housing.nsw.gov.au.

the period between 1st January 2014 and 31st March 2014, in which the announcement and subsequent implementation of the lockout laws took place. The time frame of our analysis therefore utilises three quarterly periods before the announcement of the lockout laws (June 2013 – December 2013) and seven quarterly periods after the implementation of the laws (June 2014 – December 2015). Although data before June 2013 is available, we choose the 2013 June quarter as the starting point of our analysis because the data from March reports tend to be noisy due to a downturn in the Sydney real estate market during the Christmas and New Year period, which is followed by a revival in February (NSW Government: Fair Trading 2014). Utilising the data of seven periods after the implementation of the lockout laws benefits our statistical inference by providing a larger number of observations; however, including such a long period might attenuate the causal effect we hope to estimate, especially if the effect of the lockout laws exists only for a short term. To address this concern, we also conduct a robustness analysis that uses only three periods after the introduction of the lockout laws.

4.2. Flock and Control Groups

Table 1 lists the postcode areas used in our analysis as the displacement or "flock" areas. To identify the flock areas, a mixture of anecdotal evidence from the local media and community as well as data from the BOCSAR report (Menéndez et al., 2015) are used. The BOCSAR report includes Bondi and Coogee as displacement areas, but we exclude these two suburbs because their beachside demographics are substantially different from the inner-city suburbs in the "flock" group. The addition of Erskineville is motivated by the relocation of a popular entertainment venue, The Spice Cellar, from Martin Place to

Erskineville (Milton, 2015). Similarly, Waterloo is added because of the recent death of a man as a result of a coward punch (Levy, 2015).

[Insert Table 1 Here – The Flock Areas]

To evaluate the causal effect of the lockout laws, we compare the median weekly rent in the flock areas with that of the control areas, which have no major nightlife economy but are otherwise similar to the flock areas. The control group consists of postcode areas within eight kilometres of the CBD. We use this radius criterion because postcode areas outside the 8km radius are likely to have substantially different demographics to the flock group. Although the 8km radius is an arbitrary decision, we argue that it gives a control group of inner-city and surrounding suburbs that have a time trend similar to the flock group. For example, the 8km criterion excludes suburbs in the Northern Beaches, which have beachside demographics, and Chatswood, which can be regarded as having its own business district. Postcodes within a certain radius are found using FreeMapTools,[3] and verified manually by Google Maps to ensure that the average distance from the CBD to suburbs within a specific postcode area is calculated accurately. We apply the same procedure to adjacent postcodes to ensure that no postcode is wrongfully omitted or included. The mean of the distance of the suburbs within a postcode from the CBD is also added to the dataset as a control variable.

The postcodes of the lockout and other CBD areas (the SED areas) are excluded for the purpose of focusing the analysis on our main interest – the effect of the flock phenomenon, rather than the direct impact of the lockout itself on the regulated areas. Furthermore, those lockout areas have substantially higher rental prices than the areas in

[3] www.freemaptools.com

our treatment and control groups. Postcode 2026, which denotes Bondi, is also excluded from the control group, as noted above, due to its unique beachside characteristics. Figure 1 shows a map of the flock, lockout, and control areas. For the complete postcode list, see Table A1 in the Appendix.

[Insert Figure 1 Here – Flock, Control, and Lockout Postcodes]

In our analysis, a postcode area in a given quarter comprises the unit of observation. Table 2 reports the summary statistics of median weekly rent by bedroom type and treatment status. The mean of median weekly rents is calculated across postcode areas in the period from June 2013 to December 2015. The first column confirms that the weekly rent increases with the number of bedrooms. The last category – 4+ bedroom dwellings – has considerably less observations than dwellings with fewer bedrooms because of the small number of bond lodgements for larger inner-city rental dwellings. The next two columns show that approximately 16% of our observations are from the flock (treatment) group. There is no significant difference in the average rent between the flock and control groups. The average rent of the 4+ bedroom type in the flock group is considerably lower than that of the control group, but the number of observations in the 4+ bedroom type is small and the standard deviations are large. This difference is smaller when median is used instead of mean across postcode-periods.

[Insert Table 2 Here – Summary Statistics of Median Weekly Rent]

The three panels in Figure 2 illustrate time trends in weekly rent by bedroom type. Each panel shows the time trends of the flock and control areas. The time trend of the lockout group is also shown for comparison. The three time series data in each panel show reasonably similar fluctuations before the implementation of the lockout laws in

the March 2014 quarter. After the implementation of the laws, there is a noticeable plummet in the rent for one-bedroom dwellings in the treatment group for the September 2014 quarter, which appears to be a short-run effect of the lockout laws. This drop in one-bedroom rent around the September 2014 quarter is the only easily noticeable trend diversion following the implementation of the lockout laws, whereas no such effect is observed in larger dwellings, and the effect on one-bedroom dwellings appears to be short-lived, since it shows recovery shortly afterwards. Our extensive search identifies no other explanation for the rent plummet. In fact, the timing of the rent plummet coincides with rising rental rates and substantially low vacancy rates in the Sydney market (Wilson, 2014).

[Insert Figure 2 Here – Trends in Weekly Rent]

4.3. Area Characteristics

In the regression analysis, we use area characteristics to control for factors specific to each postcode area. Table 3 lists the area variables with their definitions as well as summary statistics for the treatment and control groups. The distance from the CBD is obtained as explained in the previous subsection. The percentage of renters, the percentage of single-person households, the average number of children, median age, and median weekly household income for each postcode area are collected from the 2011 Census. The number of schools is cumulatively added for all suburbs contained in the postcode based on information collected from the My School website.[4]

[Insert Table 3 Here – Area Characteristics Variables]

[4] www.myschool.edu.au

Table 3 profiles the flock and control groups, as well as the statistical difference between the two groups in the last column. The postcode areas in the control group tend to be farther from the CBD, have fewer renters and more families than the flock areas. There is no statistically significant difference in the other characteristic variables. The DID approach allows for systematic difference between the treatment and control groups, and we can obtain an unbiased estimate of the causal effect as long as the two groups follow the same time trend without the treatment, which is reasonably supported by Figure 2. Nevertheless, we conduct a number of sensitivity analyses in Section 6 to confirm that our results are robust over the selection of the two groups and not driven by a peculiar nature of data or a particular time point/postcode. Furthermore, our thorough background investigation identifies no major area-specific external shocks or policy changes during our sample period that may cause differential trends in weekly rents across the areas or bias our DID estimates.

4.4 Difference-In-Difference Approach

We start our econometric analysis with a simple linear regression model, as a benchmark, that compares the flock and control groups after the implementation of the lockout laws. In this simple model (ex post OLS), the median weekly rent of postcode area *i* in quarter *t*, which we denote $MedWeeklyRent_{it}$, is explained by

$$(1) \qquad MedWeeklyRent_{it} = \alpha Flock_i + X_i\beta + \sum_{j=Sep14}^{Dec15} \tau_j \, I[t = j] + \epsilon_{it} \, ,$$

where $Flock_i$ is an indicator variable for the postcodes in the flock group, X_i is a vector of the other area characteristics variables, $I[t = j]$ is an indicator function for quarter *j*, (α, β, τ) is a set of parameters to be estimated, and ϵ_{it} is an error term. Data from June

2014 to December 2015 quarters are used in this regression, with June 2014 being the reference period. The inclusion of the quarter dummies is important because the strong demand for housing since 2012 has led to a steadily increasing trend in rental prices Sydney-wide (Wilson, 2014). We have also attempted median rent in the log for the dependent variable, but the results are similar, with slightly worse fit than the non-log results.

The parameter of interest in Equation (1) is α. However, it is unlikely to yield an unbiased estimate of the causal effect of the flock phenomenon because the flock areas are not identical to the control areas even without the lockout laws, and the estimator is thus plagued by a number of confounding factors that cannot be fully captured by the limited number of area characteristics variables we have in (1) (e.g., see Breen et al., 2011, for the potential relationship between alcohol-related crime and community characteristics). By the same token, the comparison of weekly rents in the flock areas for the periods before and after the lockout laws does not produce a reliable causal estimate because it is also biased by confounding time trends in crime (Leung et al., 2015) and trends in the housing market (Wilson, 2014). To obtain credible estimates, we rely on a quasi-experimental research design in which the following DID model is applied to the sample, which contains both pre- and post-periods (June 2013 to December 2015 without March 2014):

(2) $$MedWeeklyRent_{it} = \alpha Flock_i + \delta Flock_i * Post_t + \sum_{j=Sep13}^{Dec15} \tau_j I[t = j] + \epsilon_{it} ,$$

where $Post_t$ is an indicator variable for the quarter periods after the implementation of the lockout laws, and δ is the parameter of interest, which gives us the causal estimate

of the flock phenomenon. We also estimate two variants of (2): one that includes $X_i\beta$, and one that includes postcode fixed effects. These two refinements attempt to control for the time-invariant characteristics of each area for a potentially better statistical inference, although there is a possibility that including additional controls adds irrelevant parameters and reduces statistical efficiency. Hence, there is no clear order among the three DID specifications, but they all provide unbiased causal estimates of the flock phenomenon as long as the common trend assumption is satisfied.

5. Results

5.1. Overall Effect

We first report the regression results of the pooled dataset, which includes all the observations of dwellings with 1, 2, 3, and 4+ bedrooms. Table 4 shows the results of the four regression models: [1] ex post OLS, [2] DID with no controls, [3] DID with controls, and [4] DID with fixed effects. Model [1] is based on a smaller number of observations than the other models because it only uses the time points after the implementation of the laws. In all regressions, statistical inference is based on standard errors robust to heteroscedasticity and postcode-level clustering. Overall, the control variables exhibit reasonable coefficient estimates. Weekly rents increase with the number of bedrooms, the proximity to the CBD, and the median household income in the area. The positive and significant coefficients on the percentages of renters and singles reflect a relatively large demand for rental properties for these demographics. The number of children has a positive and significant coefficient, which probably reflects the demand for relatively large dwellings. The number of schools per 1,000 population exhibits a negative and significant coefficient, probably because schools are

more likely to be located in areas with lower land prices. The estimated coefficients on the quarter dummies are consistent with the housing market boom in Sydney (Wilson, 2014). Adding area characteristics to the regression increases *R*-squared from 0.739 in Model [2] to 0.814 in Model [3].

[Insert Table 4 Here – Effect of Lockout Laws – Overall Effect]

While these regression models show satisfying goodness of fit and coefficient estimates with statistical significance and expected signs, the impact of the flock phenomenon is not evident in all the models. Model [1] indicates that rents in the flock areas after the lockout laws are lower by $29.87 than in the control areas, whereas the three DID estimates are positive, attributing an increase of $15.73 to $20.53 in weekly rents to the causal effect of the flock phenomenon. However, all these estimates lack statistical significance.

5.2. Heterogeneity by Bedroom Type

The results discussed in Subsection 5.1 are derived from data that pool all types of dwellings and are appropriate only when similar magnitudes of causal effect for all bedroom types are assumed, or when we are only interested in the average causal effect over all bedroom types. In reality, renters of small houses and large houses may have substantially different preferences, hence the magnitude of the causal effect may differ by bedroom type. To address this concern, we conduct a sub-sample analysis in which we repeat the same set of regression models for the sample of different bedroom types. Table 5 summarises the results, with four panels dedicated to four bedroom types. We do not estimate the model solely for the 4+ bedroom type because the number of

observations in this category is small, and we instead combine 3 bedroom and 4+ bedroom data in Panel (D). The full results are reported in the Appendix.

[Insert Table 5 Here – Effect of Lockout Laws – By Bedroom Type]

The results in Table 5 reveal a clear contrast across bedroom types. The DID estimates for one-bedroom dwellings, which are 10% or nearly 10% significant, indicate a small, negative causal effect of the flock phenomenon. No statistically significant causal effect is found in the results for 2 bedroom dwellings. In sharp contrast with these results, the DID estimates for the 3 bedroom and 3+ bedroom dwellings are positive and highly significant. For example, the three DID models in Panel (C) indicate that the flock phenomenon has increased the weekly rent of 3 bedroom dwellings by approximately twenty dollars. The observed heterogeneous effect explains why no significant effect is found in the analysis of the overall sample in Table 4, where the negative effect of one-bedroom dwellings and the positive effect of 3+ bedroom dwellings counteract each other.

6. Robustness of Results

To confirm the credibility of the results, we conduct a number of robustness tests, the selected results of which are reported in Table 6. First, we repeat the same analysis using the period from June 2013 to December 2014, instead of June 2013 to December 2015. This is motivated by the speculation that there might be a short-run effect and a long-run effect of the lockout laws. The results in Panel (A) of Table 6 show that the negative DID estimates for one-bedroom dwellings are larger and more significant than were previously obtained and that the positive DID estimates for 3 and 3+ bedroom types are smaller and less significant. This finding is consistent with the aforementioned

observations from Figure 2, indicating that the negative impact of the flock phenomenon was only short-run and outweighed by the positive effect for larger houses, which remained after the first several quarters. Nevertheless, the contrast between the negative effect for the one-bedroom group and the positive effect for the 3+ bedroom group is still evident in this short-term analysis.

[Insert Table 6 Here – Robustness of Results]

We then repeat the regression analysis, altering the set of treatment and control postcodes. First, we conduct the analysis in which the control group is restricted to postcode areas whose centroid is within six kilometres of the CBD, instead of the initial eight kilometres. This is motivated by the fact that, as shown in Table 3, the original control areas are on average two kilometres farther from the CBD than the flock areas. When we impose the 6km restriction, the number of control postcode areas reduces from 35 to 17, and the statistical differences in area characteristics between the control and treatment areas become less significant. The results shown in Panel (B) of Table 6 confirm the robustness of our results. The DID estimates exhibit lower statistical significance than previously, but this is mainly due to the loss of observations in the control group. Nevertheless, the signs of the DID estimates are consistent with the results in Table 5 and the magnitudes tend to be even larger.

Panels (C-1) to (C-6) show the results of robustness tests in which we repeatedly move each of the six treatment group postcodes to the control group. We conduct this analysis because the classification of the treatment and control groups is somewhat subjective based on media coverage and anecdotal evidence, hence misclassification is possible. These robustness tests are also useful in checking whether our results are driven by one particular postcode. The results of this test further highlight the

robustness of our main results. Although the magnitudes of the DID estimates and their statistical significance vary across the different sets of treatment postcodes, all six experiments show a consistent pattern: there is weak evidence for a negative effect for one-bedroom dwellings and a positive, statistically significant effect for 3 and 3+ bedroom dwellings.

7. Discussion and Conclusion

In this paper, we studied the causal effect of the flock phenomenon of the Sydney lockout laws on rental prices by applying a difference-in-difference (DID) approach to postcode-level weekly rent data. Although the overall effect models find no statistically significant causal effect, sub-sample analysis reveals differential causal effects: a negative effect on one-bedroom dwellings and a positive effect on 3+ bedroom dwellings. The former effect is short-lived, while the latter is persistent, hence the positive effect on the flock areas appears to dominate in the long run. These opposite effects offset each other and consequently result in the insignificant estimate in the overall effect models. This pattern is found to be robust across alternative specifications.

Why has the flock phenomenon led to differential effects? We speculate that these differential effects have arisen from the geographical heterogeneity of dwellings of different sizes. Our analysis is at the postcode level, which is not very finely defined: our sample postcodes have a population of 13,300 on average. Consequently, the area inside each postcode may not be geographically homogeneous. As a result, it is probable that one-bedroom dwellings (typically units in an apartment building) tend to be close to main bars and entertainment strips, whereas dwellings with 3+ bedrooms (typically houses) tend to be in a quiet part of the postcode. If that is the case, the

increase in alcohol-fuelled risk, together with street noise and feelings of unsafety, is more salient to renters of one-bedroom dwellings, whereas the benefit of having a bustling entertainment strip within walking distance outweighs the disadvantages for the renters of dwellings with 3+ bedrooms.

Another possible explanation lies in the dissimilar demographics of different size dwellings. In particular, there is a high possibility that dwellings with 3+ bedrooms for rent in flock areas are share-houses due to the likely demographic of the occupants and the proximity of the dwellings to universities and other types of educational institutions. As students generally have low incomes, shared housing (usually a dwelling with three or more bedrooms) may be more economical than renting a smaller dwelling alone. The increase in nightlife in these flock areas is now an added drawcard for students and may have increased the demand for larger dwellings in these areas. There may be other explanations. Identifying the true mechanism behind the heterogeneous effect is left for the future research.

Our results highlight the importance of potential heterogeneity in the effect of geographical alcohol control policies. Our results also highlight heterogeneity in the time dimension and show contrasting short-run and long-run effects.

Our causal estimates may be biased due to the general equilibrium effect, that is, the possibility that renters move between the flock areas and the control areas and the demand of new renters for housing equilibrates between the flock areas and the control areas. This equilibrium effect affects our estimates not only by changing the rental prices in the treatment group but also by changing the rental prices in the control group in the opposite direction. Our estimates may therefore overstate the true causal effect;

however, this general equilibrium effect may be of secondary importance, and it does not affect the direction of our causal estimates.

Some of the displacement areas, such as Newtown and Enmore, are currently under consideration for the introduction of self-imposed 3:00 am lockout laws (Koziol, 2015). The lockout laws have also been considered repeatedly in Melbourne (Yahoo7, 2016) since the city adopted the laws in 2008 and abandoned the trail three months later (Brook, 2016). The state of Queensland has recently announced the lockout laws to be implemented from 1 February 2017 (Queensland Government, 2016). While the introduction of the lockout laws is likely to improve safety in these areas, its possible impact on neighbouring districts requires careful examination. The dual effect of the lockout laws we find in this paper suggests that well-designed geographically targeted alcohol control can be a cost-effective approach even when crime displacement is taken into consideration, because the Sydney lockout laws have resulted in the relocation of not only violence and crime but also nightlife entertainment hubs. The negative impact is weak and short-lived, whereas the positive effect appears to be relatively large and persistent. Our results are consistent with the report by Donnelly et al. (2016) and the evidence in the literature (Guerette and Bowers, 2009; Bowers et al., 2011; Johnson et al., 2012; Telep et al., 2014), which consistently reveals that the amount of crime displaced is far less than the amount of crime prevented in the target areas. Research also suggests a possible “diffusion of crime control benefits” to surrounding areas (Clarke and Weisburd, 1994; Bowers et al., 2011). Nevertheless, our finding of a negative flock effect also suggests that effective crime prevention in displacement areas can further enhance the social value of geographical crime policies.

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Table 1: The Flock Areas

Postcode	Suburb
2009	Pyrmont and Darling Island
2016	Redfern
2017	Waterloo and Zetland
2037	Glebe, Harold Park and Forest Lodge
2042	Enmore and Newtown
2043	Erskineville

Table 2: Summary Statistics of Median Weekly Rent

MedianWeeklyRent (AU$)	All sample areas	Flock areas	Control areas
All types			
Mean	703.7	702.4	703.9
Standard deviation	(262.7)	(198.3)	(273.1)
Number of observations	1,223	193	1,030
(A) 1 bedroom dwellings			
Mean	469.5	479.7	467.5
Standard deviation	(57.4)	(62.7)	(56.2)
Number of observations	370	60	310
(B) 2 bedroom dwellings			
Mean	629.7	672.5	622.1
Standard deviation	(75.2)	(40.9)	(77.4)
Number of observations	399	60	339
(C) 3 bedroom dwellings			
Mean	890.7	891.2	890.6
Standard deviation	(142.9)	(87.5)	(151.4)
Number of observations	371	60	311
(D) 4+ bedroom dwellings			
Mean	1,267.7	997.5	1,317.9
Standard deviation	(373.4)	(196.9)	(377.7)
Number of observations	83	13	70

Note: MedianWeeklyRent is the median weekly rental price of bonds lodged with the Renting and Strata Service Branch. Data are drawn from the period June 2013 – December 2015.

Table 3: Area Characteristics Variables

Characteristics variables	Definition	Mean: Flock	Mean: Control	Difference
KmsCBD	The mean distance from the CBD (km) of the suburbs within the postcode	3.75	5.80	-2.04 ***
%Renters	The percentage of renters in the postcode population	58.87	43.53	15.34 ***
%Singles	The percentage of single-person households in the postcode population	77.08	75.71	1.37
AvgNumChild	The average number of children per household in the postcode	1.48	1.65	-0.16 ***
MedAge	The median age of the population of a postcode	34.17	36.23	-2.06
MedIncome	The median weekly household income for the postcode	1,739	1,992	-253
SchoolsPer1000	The number of schools per 1,000 people in the postcode	0.29	0.48	-0.19

Note: All variables are defined at the time of the 2011 Census. The numbers are based on 6 flock postcode areas and 41 control postcode areas. In the last column, the results of *t* tests for the statistical difference between the two groups are reported by * p<0.1, ** p<0.05, *** p<0.01.

Table 4: Effect of Lockout Laws – Overall Effect

Dependent variable: MedianWeeklyRent	[1] Ex post OLS	[2] Dif-in-Dif No Controls	[3] Dif-in-Dif w/ Controls	[4] Dif-in-Dif Fixed Effects
Sample periods used	Jun14–Dec15	Jun13-Dec15	Jun13-Dec15	Jun13-Dec15
Bedroom type (reference: one-bedroom)				
2 Bedroom	165.23***	160.58***	160.10***	161.52***
	(8.32)	(7.41)	(8.10)	(7.77)
3 Bedroom	428.38***	421.83***	421.66***	421.14***
	(18.11)	(18.46)	(17.92)	(18.26)
4+ Bedroom	819.13***	796.58***	800.10***	812.52***
	(91.77)	(104.71)	(92.35)	(91.35)
Flock	-29.87	-16.39	-46.11	
	(29.57)	(38.05)	(35.01)	
Flock*Post (DID Estimator)		20.53	15.73	16.86
		(15.45)	(15.43)	(15.24)
KmsCBD	-18.33**		-17.57*	
	(8.60)		(8.72)	
%Renters	5.03**		4.82**	
	(1.90)		(1.83)	
%Singles	13.70*		13.87**	
	(7.25)		(6.87)	
AvgNumChild	386.50***		362.02***	
	(98.96)		(93.01)	
MedAge	5.24		4.46	
	(4.17)		(3.92)	
MedIncome	0.15***		0.15***	
	(0.04)		(0.04)	
SchoolsPer1000	-42.37***		-45.19***	
	(12.37)		(12.66)	
Sep13		-5.23	-5.37	-3.92
		(9.33)	(9.24)	(9.11)
Dec13		9.15	9.48	12.46
		(8.48)	(7.76)	(7.70)
Jun14		26.37***	29.46***	31.06***
		(8.98)	(8.22)	(8.12)
Sep14	-12.46	12.42*	16.88**	16.78**
	(8.69)	(6.65)	(6.51)	(6.60)
Dec14	-1.26	23.89***	28.00***	28.47***
	(9.11)	(7.13)	(6.85)	(6.58)
Mar15	7.66	33.33***	37.70***	35.83***
	(9.85)	(9.59)	(8.61)	(7.96)
Jun15	17.15**	45.02***	46.73***	48.78***
	(8.44)	(8.55)	(7.97)	(7.70)
Sep15	4.74	28.00***	34.76***	33.22***
	(10.89)	(8.20)	(8.42)	(7.82)
Dec15	34.50***	57.44***	64.06***	62.66***
	(9.10)	(8.48)	(8.89)	(8.04)
Intercept	-1784.11**	446.49	-1753.50**	
	(703.49)	(10.36)	(666.81)	
R^2	0.821	0.739	0.814	
N	857	1, 223	1, 223	1, 223

Note: Standard errors robust to heteroscedasticity and postcode clusters are in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.

Table 5: Effect of Lockout Laws – By Bedroom Type

Dependent variable: MedianWeeklyRent	[1] OLS	[2] Dif-in-Dif No Controls	[3] Dif-in-Dif w/ Controls	[4] Dif-in-Dif Fixed Effects
Sample periods used	Jun14–Dec15	Jun13-Dec15	Jun13-Dec15	Jun13-Dec15
(A) one-bedroom dwellings				
Flock	-26.76	17.49	-17.96	
	(22.95)	(26.63)	(22.74)	
Flock*Post (DID Estimator)		-7.51	-11.45*	-9.89
		(5.89)	(6.41)	(5.94)
Period Dummies	YES	YES	YES	YES
Other control variables	YES		YES	
Postcode fixed effects				YES
R^2	0.309	0.030	0.326	
N	261	370	370	370
(B) 2 bedroom dwellings				
Flock	-1.40	48.78**	-5.38	
	(19.21)	(19.56)	(19.98)	
Flock*Post (DID Estimator)		2.14	1.74	2.67
		(8.74)	(8.75)	(8.51)
Period Dummies	YES	YES	YES	YES
Other control variables	YES		YES	
Postcode fixed effects				YES
R^2	0.598	0.124	0.610	
N	278	399	399	398
(C) 3 bedroom dwellings				
Flock	-57.50	-15.60	-78.88*	
	(39.75)	(39.89)	(40.73)	
Flock*Post (DID Estimator)		22.57**	19.45**	19.41**
		(10.21)	(9.58)	(9.04)
Period Dummies	YES	YES	YES	YES
Other control variables	YES		YES	
Postcode fixed effects				YES
R^2	0.623	0.034	0.648	
N	257	371	371	371
(D) 3 and 4+ bedroom dwellings				
4+ Bedroom	355.38***	377.71***	363.90***	388.01***
	(64.65)	(98.38)	(68.48)	(69.14)
Flock	-83.65	-119.07*	-127.81**	
	(60.08)	(65.76)	(61.44)	
Flock*Post (DID Estimator)		78.68***	62.28**	55.86**
		(26.88)	(24.90)	(26.86)
Period Dummies	YES	YES	YES	YES
Other control variables	YES		YES	
Postcode fixed effects				YES
R^2	0.650	0.363	0.662	
N	355	469	469	469

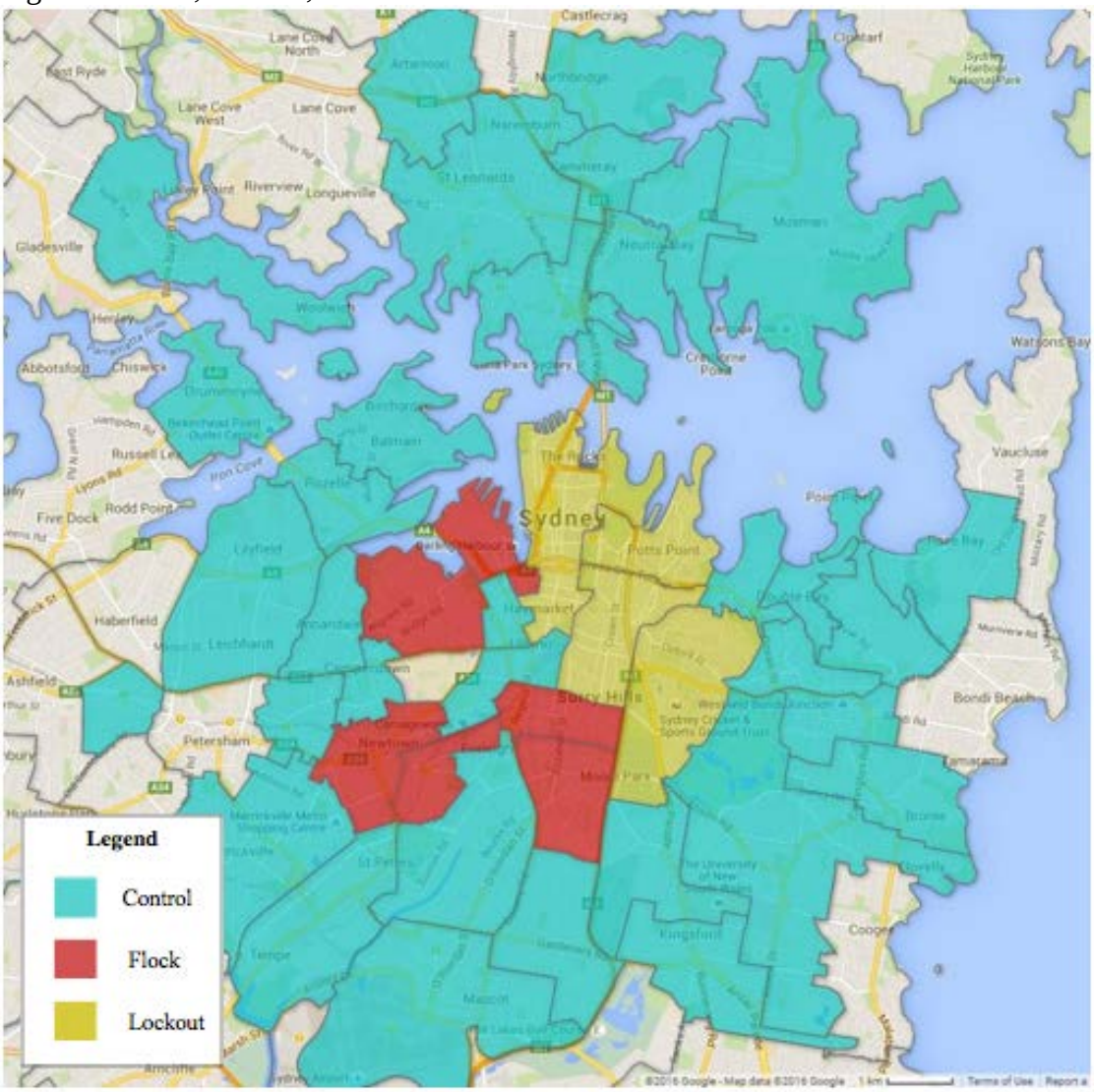
Note: Standard errors robust to heteroscedasticity and postcode clusters are in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.

Table 6: Robustness of Results

Dep.var. MedianWeeklyRent	1 Bedroom	2 Bedroom	3 Bedroom	3+ Bedroom
(A) Short-term effect (sample period Jun13-Dec14, instead of Jun13-Dec15)				
[2] DID No Controls	-11.29*	-1.30	12.74	46.30*
	(6.35)	(8.67)	(7.77)	(24.63)
[3] DID with Controls	-15.41**	-1.81	8.98	38.52
	(6.87)	(8.84)	(8.34)	(23.13)
[4] DID with Fixed Effects	-14.23**	-1.60	7.92	49.58**
	(6.18)	(8.65)	(7.70)	(23.25)
(B) Excluding areas whose KmsCBD >= 6km				
[2] DID No Controls	-9.97	3.74	25.67*	80.67**
	(6.94)	(9.77)	(14.70)	(30.76)
[3] DID with Controls	-14.47*	2.20	22.13*	42.56
	(8.27)	(10.33)	(12.55)	(26.61)
[4] DID with Fixed Effects	-10.24	3.74	23.79*	34.38
	(6.99)	(9.75)	(11.82)	(28.15)
(C-1) Moving Postcode 2009 from Flock to Control group				
[2] DID No Controls	-8.19	4.41	23.18**	83.12***
[3] DID with Controls	-11.92	4.07	20.28*	67.82**
[4] DID with Fixed Effects	-10.58	4.98	20.30**	62.74**
(C-2) Moving Postcode 2016 from Flock to Control group				
[2] DID No Controls	-2.41	0.47	27.38***	82.12***
[3] DID with Controls	-5.84	0.09	24.25**	66.71**
[4] DID with Fixed Effects	-4.65	1.01	24.23***	58.42*
(C-3) Moving Postcode 2017 from Flock to Control group				
[2] DID No Controls	-5.24	6.07	23.20**	85.26***
[3] DID with Controls	-9.20	5.71	20.11*	67.44**
[4] DID with Fixed Effects	-7.59	6.62	20.17*	62.63**
(C-4) Moving Postcode 2037 from Flock to Control group				
[2] DID No Controls	-10.56*	-6.32	14.19*	48.01**
[3] DID with Controls	-13.95**	-6.71	11.24	32.91**
[4] DID with Fixed Effects	-12.84**	-5.79	11.12	21.77
(C-5) Moving Postcode 2042 from Flock to Control group				
[2] DID No Controls	-9.52	5.06	21.42*	69.58*
[3] DID with Controls	-13.01*	4.67	18.28*	54.90
[4] DID with Fixed Effects	-11.79*	5.58	18.24*	56.97
(C-6) Moving Postcode 2043 from Flock to Control group				
[2] DID No Controls	-7.63	2.61	21.89*	85.18***
[3] DID with Controls	-11.88	2.25	18.78*	68.22**
[4] DID with Fixed Effects	-10.00	3.16	18.79*	61.61**

Note: Standard errors robust to heteroscedasticity and postcode clusters are in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.

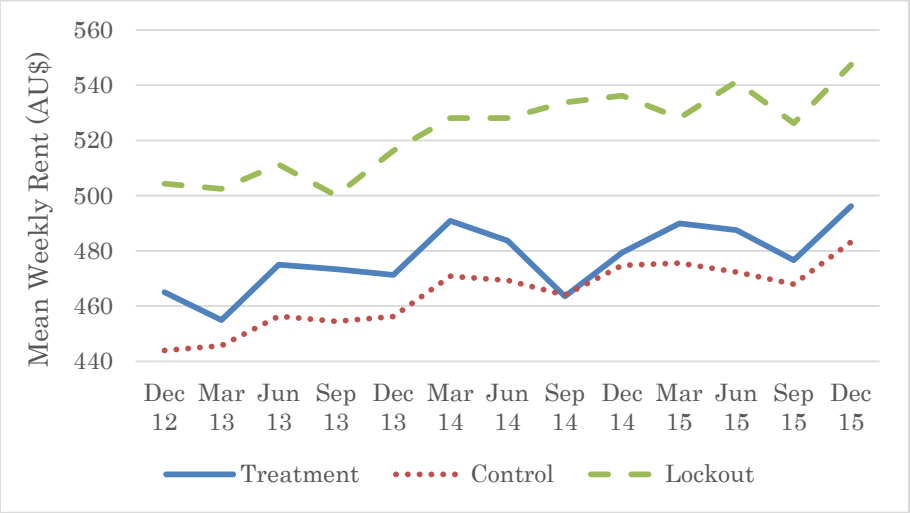
Figure 1: Flock, Control, and Lockout Postcodes



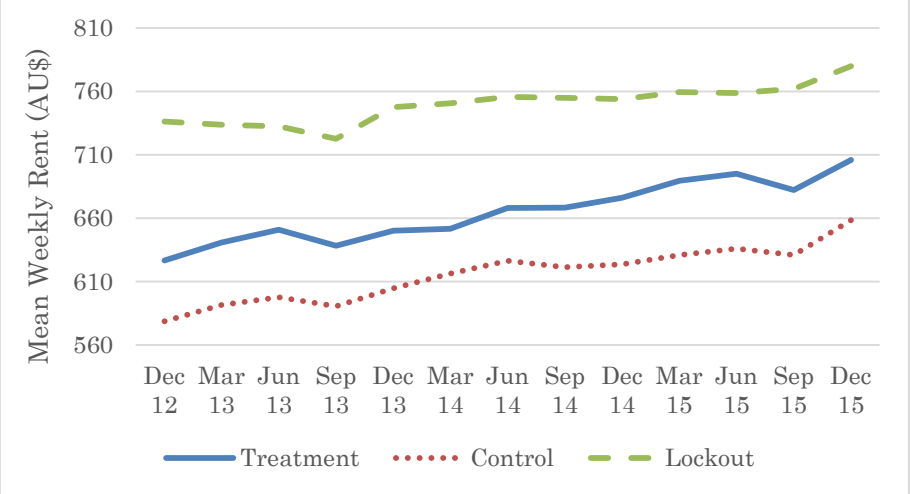
Note: The actual lockout area (the Kings Cross and CBD entertainment precincts) is smaller than the lockout postcodes shown in the figure. The difference arises because of the use of postcode level data in this paper.

Figure 2: Trends in Weekly Rent

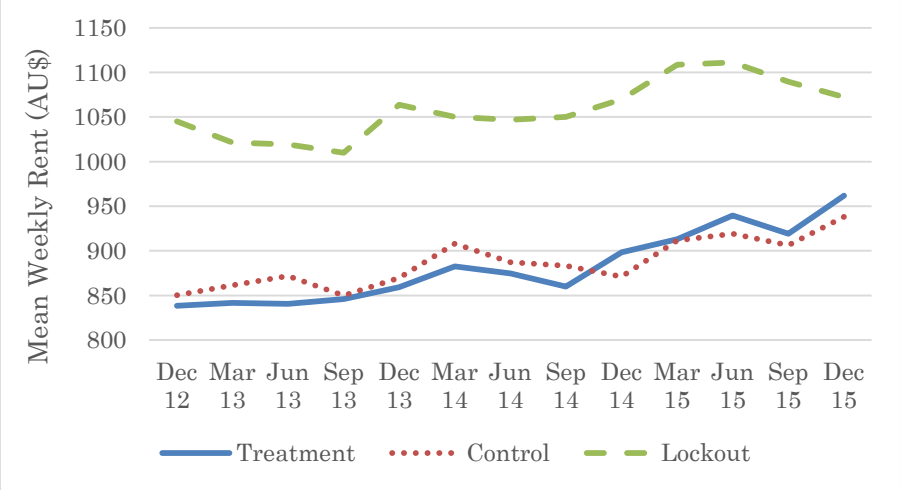
(A) One-bedroom dwellings



(B) 2 bedroom dwellings



(C) 3 bedroom dwellings



Note: The figures show time trends in the mean of the postcode-level median weekly rental price data for three groups: the displacement postcodes (Treatment), control postcodes (Control), and postcodes directly restricted by the lockout laws (Lockout).

Appendix

Table A1: List of Postcode Areas Used in Analysis

Postcode	Flock/Control/Lockout	Suburbs Represented by Postcode
2000	Lockout	Barangaroo, Darling Harbour, Dawes Point, Haymarket, Millers Point, Parliament House, Sydney, Sydney South and The Rocks
2007	Control	Ultimo
2008	Control	Chippendale, Darlington, Golden Grove
2009	Flock	Darling Island, Pyrmont
2010	Lockout	Darlinghurst, Surry Hills and Taylor Square
2011	Lockout	Elizabeth Bay, HMAS Kuttabul, Kings Cross, Potts Point, Rushcutters Bay and Woolloomooloo
2015	Control	Alexandria, Beaconsfield, Eveleigh
2016	Flock	Redfern
2017	Flock	Waterloo, Zetland
2018	Control	Eastlakes, Rosebery
2020	Control	Mascot, Sydney Domestic Airport, Sydney International Airport
2021	Lockout	Centennial Park, Moore Park and Paddington
2022	Control	Bondi Junction, Bondi Junction Plaza, Queens Park
2023	Control	Bellevue Hill
2024	Control	Bronte, Charing Cross, Waverley
2025	Control	Woollahra
2027	Control	Darling Point, Edgecliff, HMAS Rushcutters, Point Piper
2028	Control	Double Bay
2029	Control	Rose Bay
2031	Control	Clovelly, Clovelly West, Randwick, St Pauls
2032	Control	Daceyville, Kingsford
2033	Control	Kensington
2037	Flock	Forest Lodge, Glebe
2038	Control	Annandale
2039	Control	Rozelle
2040	Control	Leichhardt, Lilyfield
2041	Control	Balmain, Balmain East, Birchgrove
2042	Flock	Enmore, Newtown
2043	Flock	Erskineville
2044	Control	St Peters, Sydenham, Tempe
2047	Control	Drummoyne
2048	Control	Stanmore, Westgate
2050	Control	Camperdown
2060	Control	HMAS Platypus, HMAS Waterhen, Lavender Bay, McMahons Point, North Sydney, North Sydney Shopping World, Waverton
2061	Control	Kirribilli, Milsons Point
2062	Control	Cammeray
2063	Control	Northbridge
2064	Control	Artarmon
2065	Control	Crows Nest, Gore Hill, Greenwich, Naremburn, Royal North Shore Hospital, St Leonards, Wollstonecraft
2088	Control	Mosman
2089	Control	Neutral Bay, Neutral Bay Junction
2090	Control	Cremorne, Cremorne Point
2110	Control	Hunters Hill, Hunters Hill West, Woolwich
2130	Control	Summer Hill
2204	Control	Marrickville, Marrickville Metro, Marrickville South

Table A2: Effect of Lockout Laws – One-Bedroom Dwellings

Dependent variable: MedianWeeklyRent	[1] OLS	[2] Dif-in-Dif No Controls	[3] Dif-in-Dif w/ Controls	[4] Dif-in-Dif Fixed Effects
Sample periods used	Jun14–Dec15	Jun13-Dec15	Jun13-Dec15	Jun13-Dec15
Flock	-26.76	17.49	-17.96	
	(22.95)	(26.63)	(22.74)	
Flock*Post (DID Estimator)		-7.51	-11.45*	-9.89
		(5.89)	(6.41)	(5.94)
KmsCBD	-12.00		-11.84	
	(7.68)		(7.42)	
%Renters	2.61		2.73	
	(1.62)		(1.68)	
%Singles	-3.52		-3.11	
	(5.41)		(5.12)	
AvgNumChild	136.35		133.73	
	(88.39)		(92.16)	
MedAge	-3.91		-3.72	
	(3.80)		(3.65)	
MedIncome	0.05		0.05	
	(0.03)		(0.03)	
SchoolsPer1000	-27.58**		-31.80**	
	(12.22)		(13.49)	
Sep13		-1.83	-0.95	-1.67
		(4.65)	(4.80)	(9.11)
Dec13		-0.78	0.98	0.88
		(5.82)	(5.04)	(4.54)
Jun14		13.49**	17.51***	16.54***
		(5.65)	(6.50)	(5.66)
Sep14	-5.98	5.80	11.55*	10.40*
	(4.40)	(6.65)	(6.15)	(5.17)
Dec14	3.11	17.29***	20.67***	20.26***
	(5.88)	(5.84)	(5.45)	(4.59)
Mar15	5.10	19.73***	22.51***	20.50***
	(4.72)	(3.76)	(4.38)	(3.80)
Jun15	3.31	16.57**	20.76***	20.27***
	(5.77)	(6.95)	(7.18)	(6.39)
Sep15	-0.09	11.20	17.84**	14.08***
	(5.36)	(6.69)	(6.93)	(5.80)
Dec15	16.36**	27.17***	34.20***	30.13***
	(5.79)	(5.99)	(7.01)	(6.08)
Intercept	521.16	456.57	464.45	
	(543.09)	(10.69)	(523.72)	
R^2	0.309	0.304	0.326	
N	261	370	370	370

Note: Standard errors robust to heteroscedasticity and postcode clusters are in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.

Table A3: Effect of Lockout Laws – 2 Bedroom Dwellings

Dependent variable: MedianWeeklyRent	[1] OLS	[2] Dif-in-Dif No Controls	[3] Dif-in-Dif w/ Controls	[4] Dif-in-Dif Fixed Effects
Sample periods used	Jun14–Dec15	Jun13-Dec15	Jun13-Dec15	Jun13-Dec15
Flock	-1.40	48.78**	-5.38	
	(19.21)	(19.56)	(19.98)	
Flock*Post (DID Estimator)		2.14	1.74	2.67
		(8.74)	(8.75)	(8.51)
KmsCBD	-32.10***		-30.59***	
	(6.33)		(6.17)	
%Renters	1.09		1.27	
	(1.71)		(1.64)	
%Singles	2.79		1.78	
	(4.81)		(4.73)	
AvgNumChild	110.20		93.51	
	(78.35)		(74.49)	
MedAge	4.20		3.29	
	(4.12)		(3.90)	
MedIncome	0.04		0.05*	
	(0.03)		(0.03)	
SchoolsPer1000	-16.13		-19.30	
	(13.07)		(13.15)	
Sep13		-7.75	-7.67	-7.75
		(4.88)	(4.92)	(4.87)
Dec13		6.02	6.26	5.25
		(4.89)	(5.07)	(4.81)
Jun14		26.93***	27.37***	26.85***
		(5.65)	(5.78)	(5.59)
Sep14	-4.38	22.62***	22.98***	22.54***
	(4.79)	(6.38)	(6.42)	(6.31)
Dec14	-1.21	25.65***	26.17***	25.57***
	(5.05)	(6.23)	(6.32)	(6.17)
Mar15	6.65	33.86***	33.82***	31.29***
	(4.45)	(7.85)	(7.47)	(6.74)
Jun15	11.87**	38.93***	39.22***	38.85***
	(5.25)	(6.84)	(6.88)	(6.80)
Sep15	6.64	32.80***	34.17***	32.72***
	(4.78)	(5.28)	(5.51)	(5.26)
Dec15	35.06***	61.74***	62.34***	59.18***
	(3.77)	(7.69)	(7.35)	(6.54)
Intercept	142.71	598.18	228.42	
	(528.62)	(13.25)	(515.37)	
R^2	0.598	0.124	0.610	
N	278	399	399	398

Note: Standard errors robust to heteroscedasticity and postcode clusters are in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.

Table A4: Effect of Lockout Laws – 3 Bedroom Dwellings

Dependent variable: MedianWeeklyRent	[1] OLS	[2] Dif-in-Dif No Controls	[3] Dif-in-Dif w/ Controls	[4] Dif-in-Dif Fixed Effects
Sample periods used	Jun14–Dec15	Jun13-Dec15	Jun13-Dec15	Jun13-Dec15
Flock	-57.50	-15.60	-78.89*	
	(39.75)	(39.89)	(40.73)	
Flock*Post (DID Estimator)		25.57**	19.45**	19.41**
		(10.21)	(9.58)	(9.04)
KmsCBD	-33.31**		-35.03***	
	(12.71)		(11.69)	
%Renters	9.27***		9.32***	
	(2.50)		(2.42)	
%Singles	16.57**		16.13**	
	(8.05)		(7.54)	
AvgNumChild	641.88***		654.40***	
	(127.87)		(123.44)	
MedAge	19.01***		18.38***	
	(5.58)		(5.07)	
MedIncome	0.15***		0.16***	
	(0.03)		(0.03)	
SchoolsPer1000	-51.72***		-53.50***	
	(13.45)		(13.86)	
Sep13		-16.89	-19.64*	-19.82**
		(10.22)	(10.45)	(9.25)
Dec13		1.26	5.59	10.65
		(17.14)	(16.44)	(16.26)
Jun14		14.37	23.08**	23.63**
		(11.67)	(11.42)	(10.71)
Sep14	-14.82	9.45	8.26	7.27
	(15.30)	(13.45)	(14.27)	(13.58)
Dec14	-12.81	4.93	10.35	17.08
	(13.13)	(11.59)	(10.90)	(10.83)
Mar15	19.43	41.57**	42.55***	44.21***
	(14.23)	(14.63)	(14.71)	(14.59)
Jun15	35.13**	52.15***	58.39***	62.11***
	(15.99)	(13.64)	(13.62)	(13.35)
Sep15	17.91	37.76***	41.18***	40.33***
	(14.90)	(13.09)	(13.60)	(12.89)
Dec15	51.31***	72.24***	74.19***	75.04***
	(12.88)	(11.80)	(11.14)	(10.87)
Intercept	-2597.77***	869.28	-2586.72***	
	(635.09)	(25.59)	(580.64)	
R^2	0.623	0.038	0.648	
N	257	371	371	371

Note: Standard errors robust to heteroscedasticity and postcode clusters are in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.

Table A5: Effect of Lockout Laws – 3 and 4+ Bedroom Dwellings

Dependent variable: MedianWeeklyRent	[1] OLS	[2] Dif-in-Dif No Controls	[3] Dif-in-Dif w/ Controls	[4] Dif-in-Dif Fixed Effects
Sample periods used	Jun14–Dec15	Jun13-Dec15	Jun13-Dec15	Jun13-Dec15
Bedroom type (reference: 3 bedroom)				
4+ Bedroom	355.38***	377.71***	363.90***	388.01***
	(64.65)	(98.38)	(68.48)	(69.14)
Flock	-83.65	-119.07*	-127.81**	
	(60.08)	(65.76)	(61.44)	
Flock*Post (DID Estimator)		78.68***	62.28**	55.86**
		(26.88)	(24.90)	(26.86)
KmsCBD	-7.86		-14.42	
	(22.25)		(19.53)	
%Renters	11.01***		10.74***	
	(3.93)		(3.40)	
%Singles	41.01***		35.47***	
	(14.63)		(12.69)	
AvgNumChild	864.17***		817.82***	
	(176.79)		(160.00)	
MedAge	11.57		12.84*	
	(7.67)		(6.70)	
MedIncome	0.37***		0.33***	
	(0.09)		(0.08)	
SchoolsPer1000	-81.18***		-76.08***	
	(22.63)		(20.48)	
Sep13		-11.14	-9.83	-7.09
		(30.11)	(26.58)	(26.37)
Dec13		11.72	15.34	29.25*
		(19.05)	(16.33)	(16.37)
Jun14		26.10	37.08***	43.03***
		(16.32)	(13.00)	(12.09)
Sep14	9.21	-3.09	15.37	14.58
	(20.75)	(20.88)	(16.66)	(16.60)
Dec14	29.05	16.12	34.31*	42.10**
	(24.94)	(21.43)	(19.19)	(18.89)
Mar15	45.38*	33.41	48.66**	51.72**
	(26.38)	(25.71)	(20.95)	(20.89)
Jun15	65.94***	63.00***	72.06***	79.93***
	(20.24)	(18.83)	(16.30)	(15.69)
Sep15	41.34	27.07	45.50*	50.82**
	(26.62)	(28.11)	(24.16)	(23.57)
Dec15	88.17***	69.32***	90.45***	93.20***
	(28.23)	(24.43)	(23.75)	(21.87)
Intercept	-5201.27***	878.27	-4624.70***	
	(1423.39)	(31.44)	(1206.85)	
R^2	0.650	0.363	0.662	
N	355	469	469	469

Note: Standard errors robust to heteroscedasticity and postcode clusters are in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.