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Placing Public Housing: Announcement Effects of New Builds in More and Less Expensive Neighbourhoods*

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This study investigates the differential effect of the announcement of new public housing locations on property prices in relatively more expensive and less expensive neighbourhoods, taking advantage of a single unanticipated government announcement of new builds in multiple locations in the Australian Capital Territory. We apply three different approaches to control for the quality differences of properties, to be used alongside the difference-in-differences method. We find the announcement dampens property prices in more expensive host neighbourhoods but not in less expensive ones, and when the host neighbourhoods are treated as one, we find that there is no impact, hence masking this heterogeneous impact. Our findings will help policy-makers make important decisions around equity and efficiency in the provision of public housing across regions.

I Introduction

For decades, governments in many countries have had a policy of constructing public housing to provide shelter for low-income households under subsidised arrangements (Nourse, 1963). Public housing, once constructed, remains a feature of the host neighbourhood—suburbs where new public housing is to be located (Glaeser & Quigley, 2009). Whether the local residents view this as a positive or negative feature can be seen in changes in their preferences towards living in that neighbourhood. A positive view would be reflected in a willingness to pay higher prices for houses in the neighbourhood, and a negative view would be

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reflected in a willingness to accept a lower sale price when moving out of the neighbourhood (e.g. see Bayer et al., 2007; Ellen et al., 2007; Diamond, 2016). Hence, we examine the impact of new public housing by comparing changes in the capitalised value of properties in host neighbourhoods with changes in adjacent neighbourhoods before and after an announcement that new public housing will be built.

More specifically, this paper examines whether the impact of the *announcement* of new public housing on property prices differs between host neighbourhoods that are more expensive than surrounding neighbourhoods and host neighbourhoods that are less expensive than surrounding neighbourhoods. The paper argues, in line with Baum-Snow and Marion (2009) and Diamond and McQuade (2019), that a differential impact of non-trivial magnitude should be taken into consideration for the purposes of equity and efficiency when choosing public housing locations.

The announcement examined in our paper occurred on 15 March 2017, outlining the construction of new public housing in five locations in Canberra, the capital city of Australia, located in the Australian Capital Territory (ACT). It was reported in the *Canberra Times* on the same day as follows (Lawson, 2017):

> The ACT government has unveiled sites for 141 new public housing townhouses and apartments in five suburbs in Canberra's south.The sites, most of which will have about 30 homes, are on land zoned for community facilities, a zone the government says allows supportive housing.The sites are in Monash, Holder, Chapman, Mawson and Wright. Most of the developments are groups of single-storey, two-bedroom townhouses, except at Stapylton Street, Holder, where the plan includes apartments.

This was not well received by many local residents, who were surprised by the announcement. On 3 April 2017 an article in the Canberra Times (Burgess, 2017a) headlined 'ACT government miscalculated community reaction to public housing' reported fierce criticism from the locals in some of the host neighbourhoods with regard to the location of the new public housing. Some host neighbourhood residents marched 15 kilometres for 3 h to the ACT Legislative Assembly on 6 May 2017 to deliver a petition protesting these unexpected proposed public housing developments (Burgess, 2017d).

Newspaper articles in the months following the announcement indicate that residents felt there had been no consultation (Sibthorpe, 2017), and that they objected to the use of the land for public housing rather than community facilities, as the zoning would suggest (Burgess, 2017b), and the loss of green space in their neighbourhood (Burgess, 2017a). Furthermore, residents complained about the lack of sufficient amenities and public transport in the area to support the new public housing tenants (Burgess, 2017d). Chief Minister Barr of the ACT Government dismissed the residents' concerns as thinly veiled NIMBY-ism (see, Burgess, 2017a,c). By the end of 2018 (where our data ends), the projects were still on foot although construction work had not yet begun.[1]

We grouped the five neighbourhoods where the public housing will be located into relatively more expensive and relatively less expensive neighbourhoods than the surrounding neighbourhoods where the public housing would not be located. A neighbourhood is classed as more (less) expensive if its average property price is higher (lower) than the surrounding neighbourhoods in the ACT. This creates an ideal situation to examine the differential impact of a public housing announcement on property prices, depending on whether the public housing is located in a relatively more or less expensive neighbourhood.

We set up difference-in-differences (DiD) models where in one such model, we have the treatment group (host neighbourhoods) more expensive than the control group (surrounding ACT neighbourhoods), and in another model, we have the treatment group less expensive than the control group. Hence, we leverage the relative price levels

[1] For further reactions of locals to the sudden announcement, please see Burgess (2017c,d) and other referred newspaper articles included in Appendix S1.

across the selected sample of neighbourhoods and examine whether there is any significant difference in the impact of public housing on the property prices of the more and less expensive host neighbourhoods.

Previous studies examining the impact of public housing that used a DiD method include Ellen et al. (2001), Schwartz et al. (2006), Woo et al. (2016) and Diamond and McQuade (2019). The challenges in identifying the impact outlined in those studies were: (i) separating this impact on property prices from the impacts of myriad other factors—such as location, general inflationary effects and the structural attributes of properties—and obtaining an accurate set of counterfactual prices for surrounding properties as if there had been no public housing announcement; and (ii) averting potential endogeneity bias, which may occur if the timing of the announcement and the location of the public housing had been anticipated by the residents of the host neighbourhoods or if the local government's decisions were correlated with any unobserved factors in property prices. The paper addresses these challenges by: leveraging the attributes of the chosen locations that are conducive to the purpose of the study, using an actual transaction-based property price dataset containing important price-determining characteristics of properties, and devising an appropriate methodology for estimating the impact. Within this broad empirical strategy built on DiD, we estimate the public housing announcement impact using three different approaches, each setting out a different way of controlling for quality differences between properties sold in the pre- and post-announcement periods and between treatment and control neighbourhood properties.

It should be noted that the timing of the announcement and the ACT government's choice of locations for the public housing would have been non-random. The premise of our analysis is that substantial uncertainty exists with regard to the exact timing of the announcement and the exact locations where the public housing will be constructed. Studies examining the impact of public housing on property prices that have a similar premise include Schwartz et al. (2006), Diamond and McQuade (2019) and Almagro et al. (2023). In the context of the ACT government's announcement, the idiosyncratic factors include: what policy mandate the political party in government has; the availability of funding; the macroeconomic condition of the country; the demography of the city, including how many people meet the eligibility criteria for public housing; and the state of housing and rental markets. All these factors make it difficult for a resident of a particular neighbourhood to anticipate with any degree of certainty whether and when a public housing project will be constructed in their neighbourhood. Hence, the public housing announcement can be considered a shock to the property owners, allowing us to examine the impact in a quasi-experimental framework.

There are only a few studies that have investigated the *differential* impact of public housing on surrounding property prices depending on the existing situation of the host neighbourhood, including Baum-Snow and Marion (2009), Woo et al. (2016) and Diamond and McQuade (2019). While our study belongs beside these few studies, it distinguishes itself from them in a number of ways and therefore complements the findings in the literature. First, our paper examines the impact of the *announcement* that public housing is to be constructed rather than examining the impact of *actual constructed* public housing over a long period of time. The duration of the post-announcement period in our paper is only 1 year, capturing the price effect of immediate reactions to public housing being nearby. This is important as any abrupt fall in prices due to new public housing construction may lead to large consumption disruption and mortgage default, given that properties are in general highly leveraged assets (Clayton et al., 2010; Mian et al., 2013; Arslan et al., 2015).

It should be noted there is considerable uncertainty with regard to whether and when an announced project will actually be implemented, which means the total impact is not realised as a result of the announcement (Poterba, 1984), but will be stepped across the lifetime of the project from announcement to commencement, through completion and beyond (Schwartz et al., 2006). This paper only examines the impact of the announcement of the public housing project, by comparing property prices in the host and adjacent neighbourhoods 1 year before and 1 year after the announcement.

Second, the paper acknowledges that properties are extremely heterogeneous assets (Eurostat, 2013; Hill & Syed, 2016) and applies three different methods to control for quality changes, to be used in conjunction with the DiD method. Third, the set-up of our empirical analysis—concurrent public housing, of similar size and structure, announced in five locations by the same territory government in a relatively confined region, using actual transaction price data, being able to rank the locations in terms of most to least expensive neighbourhoods, and using a flexible DiD hedonic method—puts this research in a distinctive place in the literature and contributes to a 'cleaner' identification of the heterogeneous impact. Fourth, most studies on the impact of public housing on property prices have been carried out on USA or Europe; very little has been undertaken in other regions, including Australia, on which our empirical analysis focuses on.

Our results show that the announcement on new public housing has a large impact on the prices of nearby properties in relatively more expensive neighbourhoods—a fall of around 6.0 per cent in the year after the announcement. The impact is none to negligible on relatively less expensive neighbourhoods. This is a large loss for property owners in the more expensive neighbourhoods, leading to potential consumption disruption (Clayton et al., 2010; Arslan et al., 2015), and a reduction in property taxes collected by the local/territory (in this case ACT) government, resulting in a significant efficiency loss to the local economy.

The efficiency and revenue loss could be avoided by locating the public housing in relatively less expensive neighbourhoods, but that would not satisfy equity considerations as they usually have fewer local amenities. As the objective of informed social planners is to maximise social welfare at the local level, our analysis, which looks at the differential impact of public housing location, will help these policy-makers achieve their desired balance between equity and efficiency.

The rest of the paper is organised as follows. Section II discusses the relevant literature, Section III discusses the empirical strategies employed in our investigation, Section IV details the announcement and describes the data, treatment and control groups, Section V provides the empirical results, and Section VI discusses the policy implications and concludes the paper.

II Literature on the Effects of Public Housing Location

An important aspect of public housing policy is that it is place based (Glaeser & Quigley, 2009; Koster & van Ommeren, 2019). As a result, public housing and its residents may generate external effects on the host location. Positive external effects could include the new building improving the facade of a location suffering from unsightly ageing buildings (Schwartz et al., 2006), or the income level of the local residents rising as a result of increased economic activity in the neighbourhood (Diamond & McQuade, 2019). Negative external effects on the host neighbourhood may include overcrowding and increases in crime rates (Gibbons, 2004; Aliprantis & Hartley, 2015) and a crowding out effect on private rental property development (Sinai & Waldfogel, 2005; Eriksen & Rosenthal, 2010). Hence, the extent to which new public housing generates social benefits depends on how much benefit it generates for the neighbourhood.

The external effects that public housing generates for the host location are found to be conditional on the particular features of the location. For example, public housing in a high crime location may draw new residents into criminal activities (Glaeser et al., 1996), but may also reduce crime by increasing street traffic in low density regions (Schwartz et al., 2006). Baum-Snow and Marion (2009) report that new Low-Income Housing Tax Credit (LIHTC) units significantly crowd out nearby new rental construction in gentrifying neighbourhoods in the USA, but do not have such an effect in stable and declining neighbourhoods. The same type of public housing may generate different external effects depending on the location and so it is important that policy-makers consider the specifics of each location carefully when planning new public housing projects.

There has been a modest amount of research examining the impact of new public housing finding positive, negative or no effect on surrounding property prices. The studies that report that new public housing has a positive impact on neighbouring property prices include Nourse (1963) and Rabiega et al. (1984); the studies that report new public housing has a negative effect include Goetz et al. (1996) and Lee et al. (1999); and studies finding negligible or no effect include Briggs et al. (1999) and Ellen et al. (2007). A variety of factors might have impacted the findings in these studies, including compatibility between the public housing and the host neighbourhood (Nguyen, 2005), the methodology and data used (Schwartz et al., 2006) and the type of public housing, that is whether the public housing is high concentration, including high-rise buildings or not (Cummings & Landis, 1993; Aliprantis & Hartley, 2015). It is difficult to compare and contrast the findings across such diverse studies—which use a range of data, methodologies, empirical settings, types of public housing and features of the host locations—and come to a definitive conclusion on the direction of the impact of public housing on nearby property prices.[2]

[2] A number of studies examine the effect of knowledge, rumours or the announcement of different events (not, however, of the construction of public housing). For example: Bauer et al. (2017) examine the effect of the 2011 Fukushima Daiichi nuclear power plant disaster on the prices of properties near nuclear power plants in Germany; Haisken-DeNew et al. (2018) examine the effect on property prices in Melbourne (Australia) of the release of information on the quality of schools; Hyun and Milcheva (2019) examine the impact of the announcement of urban development on property prices in Seoul (South Korea); and Kiel and McClain (1995) examine the impact of a rumour of undesirable land use on property prices in Massachusetts (USA).

To obtain a like with like comparison, we examine the *differential* impact in a controlled setting—using the same theory and empirical strategy, same dataset, same definitions and empirical setting (such as what constitutes 'neighbouring properties' and the empirical hedonic models used). Only a few studies have done this, notably Baum-Snow and Marion (2009) show that the positive amenity effect of new LIHTC in declining and stable neighbourhoods results in higher property prices and that there is little or no such effect in gentrifying neighbourhoods where they find no effect on property prices. Diamond and McQuade (2019) report that the construction of LIHTC public housing in low-income neighbourhoods results in the property prices of the surrounding neighbourhoods rising, and similar events in high-income neighbourhoods results in the opposite. While our paper examines the differential impact of public housing announcement on host neighbourhoods' property prices across different locations, we distinguish ourselves from these studies in ways that will be important for policy decisions in choosing the locations for public housing.

III Methodology

Our empirical investigation uses transaction-level data in a DiD framework in conjunction with hedonic quality adjustment. In our analysis, the treatment group is comprised of properties that are located in the neighbourhoods of the ACT where the public housing will be constructed. The control groups are comprised of properties that are located in a circle centred on the combined centre of the five public housing locations, but not including treatment properties. Specifically, we use the following hedonic DiD model (Method 1):

$$y_i = \delta_0 + \delta_1\, Suburb_i + \delta_2\, Post_i + \delta_3\, Treatment_i \times Post_i + \mathbf{Z}_i\boldsymbol{\beta} + \varepsilon_i \quad (1)$$

where, for each i, y is the logarithm of the price of property, *Post* is an indicator variable that takes the value of 1 for periods after the public housing announcement and 0 otherwise. *Suburb* is an indicator variable that takes the value of 1 if the property is located in a particular suburb and 0 otherwise. *Treatment* is an indicator variable that takes the value of 1 if the property is located in a treatment suburb and 0 otherwise. **Z** is a vector of hedonic variables capturing property characteristics and features that have important implications on property prices.[3] **Z** includes lot size, the number of bedrooms, bathrooms and parking spaces, and the presence of study, separate dining room, heating, air conditioning and ensuite. ε_i is a normally distributed error term.[4]

The variable *Suburb* is expected to control for the time-invariant location-specific factors affecting property prices. The variable *Post* is expected to capture the location invariant price changes between pre- and post-announcement periods. As a result, the DiD coefficient, δ_3, is expected to capture the effect of the public housing announcement on property prices. The method, however, has two limitations: it assumes a homogenous treatment effect and a time-invariant valuation of property characteristics, which may not hold in reality.[5,6]

Another way of estimating the announcement effect involves a two-stage procedure. In that, the first stage models (log of) property prices separately for treatment and control groups and predicts the residual for each property, using the estimated model coefficients relevant to their group. Then, using those partialled out values of the dependent variable (i.e. the residuals), in the second stage, we run a canonical DiD model. Using the previous notations, the model (Method 2) is:

$$y_{ig} = \alpha_{0g} + \alpha_{1g}\, Suburb_{ig} + \mathbf{Z_{ig}}\boldsymbol{\beta_g} + \varepsilon_{ig},\quad g = 0, 1, \qquad (2a)$$

$$\widehat{\varepsilon}_i = \delta_0 + \delta_1\, Treatment_i + \delta_2\, Post_i + \delta_3\, Treatment_i \times Post_i + \zeta_i, \qquad (2b)$$

where the subscript g indicates whether the estimation uses the treatment or control properties.

Estimating (2a) in the first stage, separately for treatment and control group properties, accounts for the differences in hedonic valuation between them. Hence, the residuals are free of any location and property specific factors while they will

[3] It is important to control for property characteristics as, in each period, only a fraction of the stock of properties is transacted, resulting in large fluctuations in the quality of sold properties across periods (Eurostat, 2013; Hill, 2013). The quality changes of the properties in both treatment and control groups may remain unobserved. If the unobserved effects on property prices remain unchanged between the pre- and post-announcement periods, the canonical DiD method will allow for the cancellation of the control and treatment property quality difference between the pre- and post-announcement periods. For a discussion on how DiD approaches address problems arising out of unobserved heterogeneity, see Parmeter and Pope (2013).

[4] There are a number of studies that have used the DiD approach to examine the impact of exogenous events on property prices, including Muehlenbachs et al. (2015), Haisken-DeNew et al. (2018) and Gibbons et al. (2019).

[5] In other words, this omits the potential interactions between **Z** and neighbourhoods and the interactions between **Z** and Post, leading to a misspecification of Equation (1).

[6] The data on the stock of housing units published by the Australian Bureau of Statistics were available for 2016 and then 2021, hence we could not control for supply change. However, our models are estimated over a short 2-year period, and given that housing stock is very slow to increase we consider the supply side of the market has not impacted the estimated impact of the announcement. Furthermore, the construction work of the announced public housing had not begun by the end of 2018 where our data end; therefore, there can be no supply effect impacting our dataset from the announced public housing.

reflect the pre- and post-announcement difference in property prices within each group. Hence, the DiD coefficient in the second stage identifies the effect of the announcement.[7]

Method 2 allows for treatment effect heterogeneity but still suffers from an important shortcoming. After a DiD estimation, it is typical to report the average treatment effect on the treated (ATT), where ATT is the treatment effect calculated at the mean values of the covariates of the treatment group. This is particularly important when using a cross-sectional data because the average characteristics of the transacted properties for the treatment group can be different from those for the control group. Furthermore, the average characteristics of the transacted properties may differ after an intervention, such as the announcement we are examining (panel data have the unique advantage of having an unchanged composition). Differences in the characteristics of the transacted properties between treatment and control groups and their changes over time will change the mean values of the covariates at which the treatment effect is estimated and consequently the estimate of the treatment effect itself. Therefore, the residual-based approach, Method 2, can provide a biased estimate of the impact of the public housing announcement on property prices.

Wooldridge (2021) proposed an approach that can overcome the shortcomings of the previous approaches. The method requires running a DiD model that includes interactions of the hedonic variables ($\mathbf{Z}$) with *Treatment* and *Post* as well as with the interaction, $Treatment \times Post$. Specifically, we run the following regression (Method 3):

$$
\begin{aligned}
y_i = {} & \delta_0 + \delta_1\, Treatment_i \\
& + \delta_2\, Post_i + \delta_3\, Treatment_i \times Post_i \\
& + (\mathbf{Z_i} - \boldsymbol{\eta})\boldsymbol{\gamma_0} + Treatment_i\, (\mathbf{Z_i} - \boldsymbol{\eta})\boldsymbol{\gamma_1} \\
& + Post_i\, (\mathbf{Z_i} - \boldsymbol{\eta})\boldsymbol{\gamma_2} + Treatment_i \\
& \times Post_i\, (\mathbf{Z_i} - \boldsymbol{\eta})\boldsymbol{\gamma_3} + \varepsilon_i \qquad (3)
\end{aligned}
$$

Here, $\boldsymbol{\eta}$ refers the vector of the mean of these hedonic characteristics for the treatment properties, and $\widehat{\delta}_3$ provides the effect of the public housing announcement at the mean values of the property characteristics. Specifically, $\widehat{\delta}_3$ identifies the average treatment effect on the treated (ATT), a policy-relevant measure of the impact of the public housing announcement on nearby property prices.

Method 3 is flexible as it allows heterogeneity in the treatment effect and allows the estimated coefficients of the characteristics to vary across groups and over time. The method calculates the treatment effect at the mean values of the characteristics of the treatment group including for the dummy variables. Therefore, Method 3 does not suffer from any of the shortcomings of the other two methods, and the ATT obtained using this method is basically the weighted average of the treatment effects for each grouping of characteristics. Appendix S2 provides a detailed derivation of Method 3 including a discussion on its usefulness in situations where heterogenous treatment effects become relevant for estimating ATT.[8]

The potential change in the valuation of hedonic characteristics does not seem important in our particular case. This is because, although across Methods 1–3 there are slight differences in the magnitudes of the impact for the more expensive treatment group, the results show sufficient similarity across these three methods. For the less expensive group, results across all three methods show no impact. Therefore, given that Method 1 is the simplest of the three methods, our discussion in Section V focuses on the results obtained from Method 1 with

[7] A similar approach has been used recently in studies on property prices in other contexts (e.g. see Banzhaf & Mangum, 2019; Breunig et al., 2019).

[8] See Wooldridge (2020) for a discussion on the unrestricted regression adjustment technique using a single cross-sectional dataset and Wooldridge (2021) for a discussion on difference-in-difference methods using pooled cross-sectional data.

only a brief discussion of the results obtained from Methods 2 and 3. All the detailed results from Methods 1–3 are provided in Appendix S4.

IV Data, and the Treatment and Control Groups

The paper uses transaction-level data consisting of actual transaction prices of properties sold in the ACT. While the data are obtained from Australian Property Monitors (APM), the original source of the data is the ACT Revenue Office.[9] The main analysis of the paper uses data consisting of 9,816 transactions of houses covering the 2-year period from March 2016 to March 2018. This sample is obtained after excluding 1,273 observations relating to properties further than 25 km from the locations of new public housing complexes (but still within the ACT) and that record extreme and implausible house prices (outside of AUD \$10,000–\$5,000,000). As we know, the announcement of new public housing in multiple locations took place on 15 March 2017; hence, our analysis uses transaction prices corresponding to 1 year before (4,844 observations) and 1 year after (4,972 observations) the announcement.

The five locations where the public housing will be constructed are in or in the close vicinity of the following suburbs: *Mawson*, *Holder*, *Monash*, *Chapman*, *Wright*, *Oxley* and *Rivett*. The five locations are indicated by the stars on the maps shown in Figure 1. In *Holder*, *Mawson* and *Wright*, the announced public housing locations are near the middle of the suburbs, surrounded by existing houses. The remaining two locations, in *Chapman* and *Monash*, are sited on the edges of these suburbs. The location on the edge of *Chapman* is just across the road from houses in *Rivett* and, similarly, the location on the edge of *Monash* is across a road and its grassed verges from houses in *Oxley*.[10] There are 556 property transactions in these seven suburbs (the five host suburbs and the two suburbs adjacent to two of the host suburbs) and these transactions are included as the treatment group in our DiD models.[11]

The property transactions that took place during our sample period in suburbs where the public housing will not be constructed are included in our DiD models in five different control groups. These control groups consist of the transactions within a series of five circles centred on the combined centre of the public housing locations, but not including treatment suburb transactions. These circles of the control groups have radii of 5, 10, 15, 20 and 25 km. Note that the suburbs in the treatment group are all located in the southern part of Canberra, which means that, in relation to the whole city, the properties in the treatment group are in close proximity to each other. More particularly, the distance of each of the five announced public housing projects from their combined centre ranges from 2.1 to 6.8 km. Our smallest (5 km-radius) control group consists of 1,569 transactions and excludes one of the announced public housing locations (the one furthest from the others, located in *Monash*), whereas our largest (25 km-radius) control group consists of 9,260 transactions and includes most of the remaining suburbs

[9] APM is a private company with a considerable presence in Australia whose business focuses on providing property data and analytics to property professionals and real estate investors. APM, similar to other property data providers in Australia, supplements this original government-sourced data with additional characteristics information obtained from many sources, such as real estate agents, online sales advertisements, newspapers and property sales magazines. Most of the dataset is publicly accessible through the Allhomes website (https://www.allhomes.com.au/ah/research).

[10] At the announced public housing locations, the border between *Chapman* and *Rivett* runs along Darwinia Terrace (a two-lane road), and the border between *Monash* and *Oxley* runs along Erindale Drive (also a two-lane road). However, throughout this manuscript, by treatment neighbourhood we indicate suburbs on which (or just at their borders) public housing would be built.

[11] Some public housing demolition was already occurring at the time of our announcement (in Red Hill and Northbourne Avenue), but the demolition sites were neither in our treatment suburbs nor part of our announcement. Both locations are not within our 5 km-radius control suburbs—the Red Hill site is outside our 5 k-radius control suburbs and the Northbourne Avenue site is outside our 10 km-radius control suburbs.

Figure 1
Announced Public Housing Locations in Treatment Suburbs and Our Five Circles of Control Groups in the Australian Capital Territory (ACT).

Notes: (1) In Figure (a)–(e), the Stars Indicate Locations of the Announced Public Housing and the Lines Outline the Treatment Suburbs. (2) Figure (f) Shows the Five Control Group Circles of Radius 5, 10, 15, 20 and 25 km Centred on the Combined Centre of the Five Announced ACT Public Housing Locations Indicated by the Five Black Dots. (3) Each Map as a Different Scale.

TABLE 1
Mean and Median Prices of Control and Treatment Group Properties Sold in Pre- and Post-Announcement Periods (in AUD $ '000)

Property Location	Pre-announcement			Post-announcement			Both periods		
	Mean	Median	*N*	Mean	Median	*N*	Mean	Median	*N*
Control-5 km	651	650	738	674	670	831	663	660	1,569
Control-10 km	628	602	1,374	661	650	1,486	645	631	2,860
Control-15 km	619	595	2,490	648	635	2,532	634	617	5,022
Control-20 km	604	579	3,713	639	622	3,858	621	600	7,571
Control-25 km	599	580	4,561	634	620	4,699	617	600	9,260
Treatment	639	625	283	649	630	273	644	630	556
More expensive suburbs	738	744	114	719	750	88	729	749	202
Wright	731	787	33	716	780	29	724	782	62
Chapman	734	700	43	749	775	30	740	749	73
Mawson	747	738	38	691	681	29	723	715	67
Less expensive suburbs	572	565	169	616	605	185	595	586	354
Holder	604	638	30	618	598	50	613	630	80
Rivett	567	552	48	603	600	64	588	571	112
Oxley	632	624	16	654	682	14	642	645	30
Monash	550	542	75	618	611	57	580	569	132
All suburbs	602	580	4,844	634	622	4,972	618	600	9,816

Note: *N* refers to the number of observations.

in the ACT. This wide range of control groups will give confidence in our findings.[12]

Table 1 shows the mean and median prices of properties and the number of observations in the control and treatment groups for pre- and post-announcement periods. The table shows that there are large differences in the pre- and post-announcement price changes between treatment and control group properties. For example, while the median price of the properties in our 10 km-radius control group increased by 8.0 per cent between pre- and post-announcement periods, it only increased by 0.8 per cent for the treatment suburbs. Similarly, the mean price of the properties in the 10 km-radius control group increased by 5.3 per cent between pre- and post-announcement periods but only increased by 1.6 per cent for the treatment suburbs.

One should, however, note that the quality of the sold properties might be different between the pre- and post-announcement periods and, in that case, the median and mean price changes would not reflect 'like with like' changes in property prices between these comparison periods. For example, the public housing announcement might have resulted in the sale of high quality properties in the treatment suburbs, then the quality-adjusted price differences between control and treatment suburbs in the post-announcement period would be higher than the corresponding (quality unadjusted) median and mean price changes.

We now turn our attention to the seven treatment suburbs and examine which of these suburbs are more expensive suburbs

[12] The geographical area of the smallest circle is 78.5 km^2 of which the treatment suburbs cover an area of 8.8 km^2 (excludes *Monash* and *Oxley*). The geographical areas of the other four circles are 314.2, 708.9, 1,256.6 and 1,963.5 square kilometres, and the treatment suburbs in these four circles cover an area of 13.3 km^2. In our study, the treatment suburbs' size are Monash-3.41 km^2, Holder-1.9 km^2, Chapman-1.9 km^2, Mawson-2.1 km^2, Wright-1.3 km^2, Reivett-1.6 km^2 and Oxley-1.1 km^2. Since, Area = π (diameter)^2, a suburb size of 3.14 km^2 indicates a treatment area of around 1 km. The sizes of our treatment areas are similar to those in previous studies, such as Schwartz et al. (2006), Ellen et al. (2007), Aliprantis and Hartley (2015) and Diamond and McQuade (2019).

and which are less expensive suburbs in relation to the control suburbs. We undertake this exercise by examining the price differences between treatment and control groups in two ways: (i) comparing their simple median and mean prices and (ii) comparing their quality-adjusted prices. As shown in Table 1, the median and mean prices of *Wright*, *Chapman* and *Mason* are significantly higher than the median and mean prices of each of our variations of control groups, placing these three suburbs in the more expensive category.

The (median and mean) prices of *Rivett*, *Oxley* and *Monash*, in contrast, are significantly lower than the prices of each of our control groups, placing these three suburbs in the less expensive category. The prices of *Holder* are close to the prices of our five control groups. The prices of *Holder* are also closer to the prices of the less expensive suburbs than they are to the prices of the more expensive suburbs. For example, the difference between the median price of *Mawson* (which has the lowest median price among the more expensive suburbs) and the median price of *Holder* is AUD$85,000, while the difference between the median price of *Oxley* (which has the highest median price among the less expensive suburbs) and *Holder* is AUD$15,000.[13]

To obtain a quality-adjusted comparison, we run hedonic regressions of log(prices) on quarterly time dummies, suburb dummies, lot size, the number of bedrooms, bathrooms and parking spaces, and the presence of a study, separate dining room, heating, air conditioning, and/or ensuite. We run five such regressions corresponding to five variations of control groups. The regressions include seven *treatment* suburb dummies. The base category for constructing these suburb dummies consists of pre-announcement transactions in the respective *control* groups. The estimated coefficients of the suburb dummies allow us to identify more expensive and less expensive suburbs compared with the control suburbs, after adjusting for quality differences.

Table 2 shows that the estimated coefficients of *Wright*, *Chapman* and *Mason* are positive and significantly different from zero (at the 5 per cent level), therefore identifying them as more expensive suburbs (consistent with the previous mean and median analysis). We find that the estimated coefficients of *Rivett*, *Oxley* and *Monash* are negative and in most cases significantly different from zero, therefore identifying them as less expensive suburbs (again consistent with the previous mean and median analysis).

The estimated coefficient of *Holder* has a negative estimated coefficient for three regressions (−5.67, −2.74 and −1.39) and a positive estimated coefficient for two (0.09, 0.47); however, none of these estimated coefficients are significant. The regression results show that the quality-adjusted prices for *Holder* are much closer to the less expensive suburbs than the more expensive suburbs, which is consistent with our previous median and mean price analysis. Considering all this, we group *Holder* with *Rivett*, *Oxley* and *Monash* in the relatively less expensive category.

For the purpose of this study, it is ideal that we are able to group the treatment suburbs into more expensive and less expensive suburbs in comparison with the control suburbs and examine the heterogeneity of the impact of the public housing announcement on property prices. From the literature, it may be expected that the public housing announcement would on average have a

[13] The table shows that while the number of properties sold dropped in the more expensive treatment suburbs, the number increased in the control and less expensive treatment suburbs. The explanation for the sales volume dynamics could be that an unanticipated fall in property prices may result in reduction in consumption, including housing service consumption, due to the negative impact on the wealth of the households. This reduction in wealth may in turn prevent the households in the impacted areas from moving out in search of the quality of housing that matches their previous level of housing service consumption (e.g. Clayton et al., 2010; Arslan et al., 2015), explaining why the property sales volumes have not increased within 1 year of the public housing announcement in the more expensive treatment suburbs. However, such an explanation of the dynamics of sales volumes should be interpreted with caution because we cannot control for other factors (such as lot size, number of bedrooms and bathrooms) that may impact sales volumes in our treatment suburbs.

TABLE 2
Quality-Adjusted Price Differences Between Control and Treatment Group Properties Sold in Pre-Announcement Period

	Control groups				
Property location	5 km-radius	10 km-radius	15 km-radius	20 km-radius	25 km-radius
Wright	12.77**	14.66***	19.09***	19.16***	19.39***
	(5.13)	(4.79)	(4.68)	(4.58)	(4.59)
Chapman	14.62***	17.36***	16.05***	17.90***	17.83***
	(2.89)	(2.70)	(2.49)	(2.44)	(2.50)
Mawson	11.54***	15.27***	14.68***	16.05***	16.21***
	(2.40)	(2.33)	(2.02)	(2.03)	(2.01)
Holder	−5.67*	−2.74	−1.39	0.09	0.47
	(3.02)	(3.02)	(3.01)	(2.98)	(3.02)
Rivett	−8.66***	−4.85**	−5.58***	−3.84**	−3.85**
	(2.10)	(2.00)	(1.96)	(1.94)	(1.96)
Oxley	–	−4.17	−6.11**	−5.24*	−5.93**
		(3.07)	(3.00)	(2.83)	(2.71)
Monash	–	−13.80***	−14.26***	−12.86***	−13.10***
		(2.83)	(2.78)	(2.75)	(2.77)

Note: (1) These are obtained by running hedonic regressions of pre-announcement log(prices) of properties on suburb dummies, quarterly time dummies and physical attributes. Five such regressions are run for five control groups and for each, the base category for the suburb dummies represents the respective control suburbs. The figures are the estimated coefficients of the suburb dummies and measure the percentage difference in the property prices between the control group suburbs and the respective treatment suburb. The figures in the parentheses are their estimated standard errors *, ** and *** refer to the significance levels 10 per cent, 5 per cent and 1 per cent, respectively. (2) The announced public housing location in *Monash* is outside the 5 km-radius circle (see Fig. 1) and the DiD regressions that use the 5 km-radius circle as the control group exclude the property transactions in *Monash* and *Oxley*.

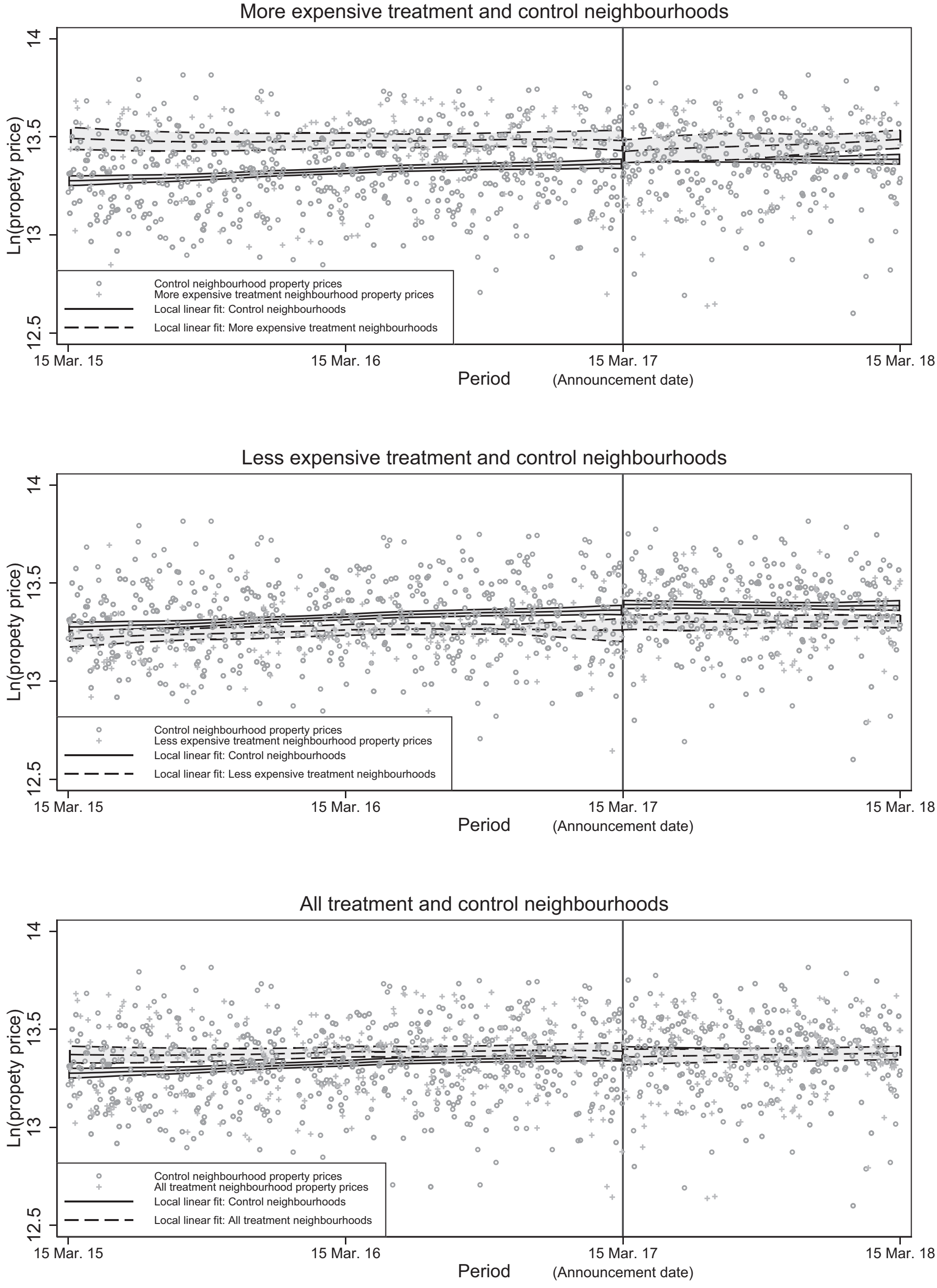
non-positive impact on the price of properties, but this may not apply across the board. There may be heterogeneity in the impact, in the sense that the effect may be larger on the property prices in more expensive suburbs than the property prices in less expensive suburbs, or it may be that the direction of the impact is different between the more and less expensive suburbs.

Figure 2 shows the scatter plots of the pre- and post-announcement property prices, the announcement being made on 15 March 2017. In the figure, we have also added group and period wise local linear fits and their 95 per cent confidence intervals.[14]

[14] Specifically, we use Kernel-weighted local linear smoothing—a non-parametric regression technique widely used in applied work. The local linear fit is particularly useful in this case because it estimates the relationship between variables in a flexible way without requiring us to specify a particular functional form.

While the linear fits of the property prices in relatively more expensive and less expensive suburbs do not have exactly the same slope as the fit of the property prices in control suburbs (of 5 km-radius), their slopes are close to each other. The linear fits indicate that during the two-year pre-announcement period there was no change in the property prices in relatively more expensive suburbs, a 2 per cent increase in relatively less expensive suburbs and a 4 per cent increase in control suburbs. Note that the plots are for actual property prices; therefore, they do not control for any quality differences in properties that might have contributed to the trends. The fact that there was no price change in the relatively more expensive suburbs before the public housing announcement indicates that it is unlikely there was any price reversion in the post-announcement period. Furthermore, the figure shows that there is a slight dip in the prices in more expensive suburbs following

FIGURE 2
Pre-Announcement Property Prices by Neighbourhood Type: Scatter Plots and Local Linear Fits (With 95 Per cent CI) (With 5 km-Radius Control Group).



the announcement but not in the control or less expensive suburbs. A similar pattern is observed for the other 4–10 km, 15 km, 20 km and 25 km radii—control groups (see Figures S1–S4).

As discussed in the methodology section, it is important to be able to control for the changes in the quality of properties between pre- and post-announcement periods to obtain a 'pure' measure of property price changes due to the announcement effect. Our dataset, combined with the methodology used, will achieve this control for the following reasons. First, the vast majority of the ACT population resides in a planned city (Canberra, the capital of Australia). Canberra is largely populated by middle-income households, and the ACT is administered by one body (the ACT Government)—all these factors contribute to a relative homogeneity of property quality, which works as a natural control for quality differences and concomitant heterogeneity of prices (Robinson, 1973; Banks & Brack, 2003; Australian Bureau of Statistics, 2019).

Homogeneity of Canberra suburbs is validated by the Socio-Economic Indexes for Areas (SEIFA), 2016. Depending on which of the SEIFA indexes is used, the more expensive suburbs are around 4.5–6.3 per cent more advantaged (or less disadvantaged) than the less expensive suburbs. For example, the SEIFA scores of the Index of Relative Socio-Economic Advantage and Disadvantage for our more expensive and less expensive suburbs are 1,137 and 1,070, respectively. The SEIFA scores of the same index for our treatment group as a whole and our control group suburbs are 1,099 and 1,089, respectively. For the whole of Australia, the SEIFA indexes have a mean of 1,000 with a standard deviation of 100. This indicates that the households living in the ACT are above the Australian average in terms of their socioeconomic conditions, but not far above the average and not far apart from each other. More details on the SEIFA scores, including how to interpret the scores, are provided in Table S1.

Second, location is an important price-determining characteristic of properties because location captures features related to amenities, such as natural bushland, parks and playgrounds, pollution, traffic congestion and distances to places of interest. In the data, the suburbs in the treatment group are all located in the southern part of Canberra, implying that, in relation to the whole city, the properties in the treatment group suburbs are in close proximity to each other, which controls for differences in some of the broader location-specific quality features, for example distance from the centre of the city. Furthermore, note that our five variations of control groups range from covering smaller areas (5 km-radius) around the treatment group suburbs to covering larger areas (25 km-radius) around these suburbs.

Third, the dataset includes information on a number of physical attributes of properties—lot size, the number of bedrooms, bathrooms and parking spaces, and the presence of a study, separate dining room, heating, air conditioning and/or ensuite. These are the physical characteristics that are typically included in hedonic regression models of property prices to control for quality differences (Eurostat, 2013; Hill, 2013) and, following this literature, we have used the same characteristics in our analysis.[15]

Fourth, despite our above controls, there may be unobserved attributes of properties that result in quality differences between pre- and post-announcement properties. Provided these differences are the same between the control and treatment properties, the DiD method will control for the unobserved quality differences between pre- and post-announcement properties.

[15] The dataset includes observations where the values corresponding to some characteristics are missing. These missing values are handled using the missing indicator method outlined in Wooldridge (2020, p. 314). Specifically, a dummy variable corresponding to a particular characteristic takes the value of 1 if the value is missing, 0 otherwise. In the regressions, the missing values in the original variable are replaced with 0 s. In addition, we estimate different variants of hedonic models where we explore different ways of including the characteristics of properties. This includes estimating hedonic models with and without missing values being included in the hedonic regressions. These variations do not make any qualitative difference with respect to our primary interest, that is the estimated DiD coefficients. The results obtained from these additional regressions are provided in Appendix S4.

All these controls allow us to obtain the price changes of the same-quality properties due to the public housing announcement. Furthermore, the fact that the five public housing projects are similar in size and will be commissioned, administered and managed by the same government agency (Housing ACT) means it is plausible to view these five public housing projects as similar in quality. Cummings and Landis (1993) argue that the design and quality of public housing structures and the management of the housing are important factors affecting whether public housing is viewed as a positive or negative feature of a neighbourhood.

Additionally, because we are able to divide the treatment suburbs into relatively more and less expensive suburbs, we can examine the differential impact of the concurrent announcement of public housing of similar size and quality in a controlled setting—using the same theory and empirical strategy, same dataset, same definitions and empirical settings (such as what constitutes ‘neighbouring properties’ and the empirical hedonic models used). Only a few studies (e.g. Baum-Snow & Marion, 2009; Woo et al., 2016; Diamond & McQuade, 2019) have examined this issue of the heterogeneous effect of public housing taking into account differences in the host locations, and none focused on the announcement effect, which means our study complements this scant literature.

V Results

(i) Cross Tables of Property Prices

We start with constructing cross tables of the mean of log prices to understand whether these simple measures provide an indication of what results we may expect to obtain (see Table 3). We construct such cross tables of the mean of log prices for treatment group properties sold in the pre- and post-announcement periods in: (a) more expensive suburbs, (b) less expensive suburbs and (c) all treatment suburbs (i.e. (a) + (b)). The table also includes the means of log prices of control group properties sold in the pre- and post-announcement periods.

The first two rows in the cross tables show the two periods—post- and pre-announcement—and the first two columns in the tables show the two property groups—treatment and control. In each of the cross tables, the last row shows the difference in the mean of log prices of properties between the pre- and post-announcement periods, and the last column shows the difference in the mean of log prices between the treatment and control properties. The difference in differences in log prices is shown at the bottom right of each of the table panels. The figures in parentheses are standard errors, and the figures in square brackets are the number of observations. These cross tables are constructed separately for our five control groups. Table 3 shows the cross table corresponding to the 10 km-radius control group (other tables are in Appendix S4).

Focusing first on the more expensive suburbs, we see that the mean of log price of the treatment group properties in the pre-announcement period is 13.49, shown in the cell at the intersection of the first row and first column. The mean of the log price of the 10 km-radius control group properties in the pre-announcement period is 13.31. So, in the pre-announcement period, the treatment group property prices are on average 17.2 per cent higher than the control group property prices. Moving one row down, to the post-announcement period prices, the property prices in the treatment group are on average 8.4 per cent higher than the property prices in the control group.

The difference between the two previous differences (17.2 per cent and 8.4 per cent) reveals that the property prices in the treatment suburbs, compared with the control suburbs, fell by approximately 8.8 per cent between the pre- and post-announcement periods. Turning now to the results for the less expensive suburbs (panel (b)), we find that the impact of the public housing announcement on nearby property prices is negligible at 2.5 per cent, which is also not statistically significant. The results are similar for the other four control groups employed in our study—5, 15, 20 and 25 km radii (Tables S2–S5).

Table 4 shows the difference in the means of log property prices between time (Post minus Pre) and group (Treatment minus Control) for all five control groups. The table

TABLE 3
Cross Tables of the Mean of Log Prices by Property Group (10 km-Radius Control Group and Treatment Group) and Period (Pre- and Post-Announcement Periods)

	Treatment (1)	Control (2)	Difference (3)
(a) More expensive suburbs			
Pre-announcement	13.486	13.314	0.172***
	(0.022)	(0.007)	(0.038)
	[114]	[1,374]	[1,488]
Post-announcement	13.452	13.368	0.084**
	(0.029)	(0.007)	(0.038)
	[88]	[1,486]	[1,574]
Post–Pre	−0.034	0.054***	−0.088***
	(0.031)	(0.011)	(0.028)
	[202]	[2,860]	[3,062]
(b) Less expensive suburbs			
Pre-announcement	13.236	13.314	−0.079*
	(0.017)	(0.007)	(0.044)
	[169]	[1,374]	[1,543]
Post-announcement	13.314	13.368	−0.054
	(0.014)	(0.007)	(0.033)
	[185]	[1,486]	[1,671]
Post–Pre	0.079***	0.054***	0.025
	(0.029)	(0.011)	(0.028)
	[354]	[2,860]	[3,214]
(c) All treatment suburbs			
Pre-announcement	13.337	13.314	0.022
	(0.015)	(0.007)	(0.067)
	[283]	[1,374]	[1,657]
Post-announcement	13.359	13.368	−0.009
	(0.014)	(0.007)	(0.041)
	[273]	[1,486]	[1,759]
Post–Pre	0.022	0.054***	−0.031
	(0.039)	(0.011)	(0.038)
	[556]	[2,860]	[3,416]

Note: (1) The figures in the square brackets refer to the number of observations. (2) The figures in parentheses refer to the standard errors (clustered at the suburb level) of the estimates. (3) *, ** and *** Refer to the significance levels 10 per cent, 5 per cent and 1 per cent, respectively.

shows that the impact on the more expensive treatment suburbs is a fall in property prices in the range of 7.0–9.3 per cent, with four out of five of these means being statistically significant at the 5 per cent level. The impact on the less expensive treatment suburbs is an increase in property prices in the range of 1.0–2.8 per cent, although none of the mean differences are significantly different from 0 at the 5 per cent level. These cross tables of mean differences provide us with an early indication of the differences between the impact of the public housing announcement on more expensive host suburbs and the impact on less expensive host suburbs. However, one should be aware that the results might be tainted by the difference in property quality between the pre- and post-announcement periods, and so we conducted quality adjustment analysis next.

(ii) Hedonic Quality Adjustments

The dataset includes information on a number of physical characteristics of properties.[16] We converted (or treated) all

[16] Table S6 provides the summary statistics of the physical characteristics across treatment and control groups by the pre- by post-announcement periods.

TABLE 4
Mean Log(Price) Differences of Control and Treatment Group Properties Sold in Pre- and Post-Announcement Periods

	Control group circles				
Treatment groups	5 km-radius	10 km-radius	15 km-radius	20 km-radius	25 km-radius
More expensive suburbs	−0.070**	−0.088***	−0.085***	−0.093***	−0.092***
	(0.030)	(0.028)	(0.027)	(0.026)	(0.026)
Less expensive suburbs	0.010	0.025	0.028	0.020	0.020
	(0.017)	(0.028)	(0.026)	(0.026)	(0.026)
All treatment suburbs	−0.061**	−0.031	−0.029	−0.037	−0.036
	(0.027)	(0.038)	(0.037)	(0.037)	(0.037)

Note: (1) The figures in parentheses refer to the standard errors (clustered at the suburb level) of the estimates. (2) *, ** and *** Refer to the significance levels 10 per cent, 5 per cent and 1 per cent, respectively.

these property characteristics as categorical variables and entered them in the hedonic part of our methods as dummy variables. The lot size is divided into quintiles, and these five groups are entered into our models as categorical variables, with the category corresponding to the first quintile treated as the base category. The number of bedrooms' ranges from 1 to 8, and the properties are grouped into four categories: 1-bedroom, 2-bedrooms, 3-bedrooms and 4 or above bedrooms. Around 33 per cent of the properties have three bedrooms, and this group is treated in the model as the base category. The number of bathrooms' ranges from 1 to 3, and the properties are grouped into the three available categories.

However, around 29 per cent of the properties have one bathroom, and this group is treated in the model as the base category. The number of parking spaces in our data ranges from 1 to 3. Around 17 per cent of properties have 1 parking space, and this is included in the model as the base category. The other variables, presence of ensuite, study, separate dining, heating, air conditioning, and/or garage are dichotomous variables, entered in the hedonic models as 1 and 0 otherwise.

The location of properties in our models is defined by suburb; there are 103 Canberra suburbs included in the data, of which 7 are treatment suburbs (including 3 more expensive and 4 less expensive suburbs) and our five control groups are formed from the remaining suburbs.

(iii) DiD Results

Table 5 provides the results of the estimated DiD coefficients for the models run separately for the more expensive, less expensive and all suburbs for each of our five control groups. Note that all regressions include a constant term, physical attributes of properties and suburb fixed effects, and that the estimated standard errors are clustered at the suburb level. These results are obtained using Method 1, the focus of our discussion in this section, which only includes a brief discussion of Methods 2 and 3 as the results across the three methods are similar (see Tables S7–S11 for detailed Method 1 results, Tables S12–S17 for the Method 2 results and Tables S18–S22 for the Method 3 results).

Focusing on the results corresponding to the 5 km-radius control group, for the more expensive suburbs, the estimated coefficient corresponding to the *Post* dummy indicates that, on average, property prices in the control suburbs increase by 5.7 per cent between the pre- and post-announcement periods. The estimated DiD coefficient indicates that the increase in the prices of treatment group properties between the pre- and post-announcement periods is 5.4 per cent lower than the control group properties. This means that while the overall prices of properties increase in the post-announcement period, the prices of the treatment group properties increase by only 0.3 per cent, indicating that the dampening effect of the new public housing

TABLE 5
Regression Results of Difference-in-Differences (DiD) Models Augmented by Controlling for Hedonic Characteristics of Properties (Method 1)

	More expensive suburbs	Less expensive suburbs	All suburbs
(a) 5 km-radius control group			
Post	0.057***	0.060***	0.057***
	(0.015)	(0.015)	(0.014)
Treatment × Post	−0.054**	0.010	−0.024
	(0.022)	(0.013)	(0.021)
Adjusted R^2	0.49	0.49	0.49
N	1,771	1,761	1,963
(b) 10 km-radius control group			
Post	0.058***	0.059***	0.058***
	(0.008)	(0.008)	(0.008)
Treatment × Post	−0.057***	0.024	−0.006
	(0.015)	(0.018)	(0.022)
Adjusted R^2	0.55	0.54	0.55
N	3,062	3,214	3,416
(c) 15 km-radius control group			
Post	0.061***	0.061***	0.061***
	(0.006)	(0.005)	(0.005)
Treatment × Post	−0.057***	0.022	−0.007
	(0.013)	(0.019)	(0.021)
Adjusted R^2	0.63	0.62	0.62
N	5,224	5,376	5,578
(d) 20 km-radius control group			
Post	0.068***	0.069***	0.068***
	(0.004)	(0.004)	(0.004)
Treatment × Post	−0.064***	0.013	−0.015
	(0.012)	(0.019)	(0.021)
Adjusted R^2	0.64	0.64	0.64
N	7,773	7,925	8,127
(e) 25 km-radius control group			
Post	0.070***	0.070***	0.070***
	(0.004)	(0.004)	(0.004)
Treatment × Post	−0.062***	0.011	−0.016
	(0.013)	(0.020)	(0.021)
Adjusted R^2	0.62	0.61	0.61
N	9,462	9,614	9,816

Note: (1) The regression models include a constant term, physical attributes of properties and suburb fixed effects (for detailed results, see Tables S7–S11). (2) The figures in parentheses refer to the standard errors of the estimates (clustered at the suburb level). (3) *, ** and *** Refer to the significance levels 10 per cent, 5 per cent and 1 per cent, respectively.

announcement on nearby property prices has been substantial. For the less expensive suburbs, the estimated DiD coefficient is small (0.010) and not significantly different from zero at the 10 per cent significance level. This indicates that the post-announcement property prices in the less expensive suburbs are not systematically affected by the announcement, contrasting with their counterparts in the more expensive suburbs.

The results from our five variations of control groups as shown in Table 5 are similar in terms of direction, magnitude and statistical significance of the estimated coefficients, demonstrating that the differential impact of the public housing announcement depends on whether the public housing is to be located in more or less expensive suburbs. For the more expensive suburbs, the average impact across our five control groups is −5.9 per cent. This is

TABLE 6
Regression Results of Difference-in-Differences (DiD) Models With Ring *Control Groups (Method 1)*

	More expensive suburbs	Less expensive suburbs	All suburbs
(a) 5–10 km ring control group			
Post	0.057***	0.059***	0.058***
	(0.008)	(0.007)	(0.007)
Treatment × Post	−0.053***	0.023	−0.007
	(0.013)	(0.017)	(0.020)
Adjusted R^2	0.62	0.59	0.59
N	1,496	1,647	1,852
(b) 10–15 km ring control group			
Post	0.067***	0.068***	0.067***
	(0.007)	(0.007)	(0.007)
Treatment × Post	−0.063***	0.014	−0.015
	(0.012)	(0.021)	(0.022)
Adjusted R^2	0.72	0.70	0.69
N	2,367	2,518	2,723

Note: (1) The regression models include a constant term, physical attributes of properties and suburb fixed effects (see Tables S23 and S24 for more detailed results using Methods 2 and 3 and both rings). (2) The figures in parentheses refer to the standard errors of the estimates (clustered at the suburb level). (3) *, ** and *** Refer to the significance levels 10 per cent, 5 per cent and 1 per cent, respectively.

significantly less than the average impact of −8.5 per cent obtained from the cross tables of mean differences that do not adjust for quality differences of properties, supporting the importance of quality adjustment in our analysis.

For the less expensive suburbs, the average impact across our five control groups is 1.5 per cent, but none of the individual estimated coefficients is significant at the 10 per cent level. When we look at all treatment suburbs—the more and less expensive suburbs—combined, we find that the impact is negative and is not statistically significant at the 10 per cent level across all our control groups. Hence, examining the average impact for all suburbs combined masks the important differential impact that we observed due to heterogeneity in the public housing host suburbs.[17]

Our finding of the heterogeneous impact of public housing is in concordance with Diamond and McQuade (2019) and Baum-Snow and Marion (2009). Diamond and McQuade (2019), similar to our findings, observe that LIHTC units in high-income neighbourhoods had a dampening effect on surrounding property prices. However, while they report LIHTC units in low-income neighbourhoods had an accelerating effect on surrounding property prices, we find that the announcement of public housing to be built in less expensive neighbourhoods has negligible to no effect on nearby property prices. Baum-Snow and Marion (2009) report that new LIHTC units in declining or stable neighbourhoods impart a positive amenity effect, leading to increases in property prices there. However, they report there is negligible or no amenity effect in gentrifying neighbourhoods. In a recent study, Blanco (2023) finds that the demolition of public housing in Chicago neighbourhoods led to a 20 per cent increase in property prices over a 10-year period in the census tracts near the demolitions, and shows that while this reduction in supply could explain the price

[17] The average impact of the public housing announcement on nearby property prices in the more expensive suburbs is found to be −5.2 per cent using method 2 and −6.0 per cent using method 3. The average impact is found to be none or negligible for the less expensive suburbs using these two methods (see Tables S12–S22).

TABLE 7
Regression Results of Difference-in-Differences (DiD) Models Augmented by Controlling for Hedonic Characteristics of Properties (Method 1) (With 5 km-Radius Control Group)

	More expensive suburbs	Less expensive suburbs	All suburbs
(a) Treatment properties within 1.0 km of public housing			
Post	0.057***	0.060***	0.058***
	(0.015)	(0.015)	(0.014)
Treatment × Post	−0.044**	0.014	−0.013
	(0.017)	(0.012)	(0.018)
Adjusted R^2	0.49	0.49	0.49
N	1,717	1,746	1,894
(b) Treatment properties within 1.5 km of public housing			
Post	0.057***	0.060***	0.058***
	(0.015)	(0.015)	(0.015)
Treatment × Post	−0.057**	0.010	−0.023
	(0.024)	(0.013)	(0.021)
Adjusted R^2	0.49	0.49	0.49
N	1,746	1,761	1,938
(c) Treatment properties within 2.0 km of public housing			
Post	0.056***	0.060***	0.057***
	(0.015)	(0.015)	(0.015)
Treatment × Post	−0.051**	0.010	−0.022
	(0.020)	(0.013)	(0.020)
Adjusted R^2	0.49	0.49	0.49
N	1,764	1,761	1,956
(d) Treatment properties within 2.5 km of public housing			
Post	0.057***	0.060***	0.057***
	(0.015)	(0.015)	(0.014)
Treatment × Post	−0.054**	0.010	−0.024
	(0.022)	(0.013)	(0.021)
Adjusted R^2	0.49	0.49	0.49
N	1,771	1,761	1,963

Note: (1) The regression models include a constant term, physical attributes of properties and suburb fixed effects (for detailed results, see Tables S25–S28). (2) The figures in parentheses refer to the standard errors of the estimates (clustered at the suburb level). (3) *, ** and *** Refer to the significance levels 10 per cent, 5 per cent and 1 per cent, respectively. We have not included the results for the treatment properties within 500 m of public housing as the number of observations is insufficient for each of the more- and less expensive suburb properties.

increases, the demographic effects of demolition suggest that demand factors also might have contributed to the increases.

Instead of the control groups being a series of circles centred on the combined centre of the five public housing locations, we also consider the control groups to be rings around each specific public housing complex. A few other papers have followed this doughnut approach, including Aliprantis and Hartley (2015) and Shoag and Veuger (2018). We consider two rings, where both rings are centred on the same centre as the base control group, with one ring including the area between 5 and 10 km radius (5–10 km ring) and the other ring including the area between 10 and 15 km radius (10–15 km ring). In other words, the rings exclude the inner circle of properties which may be regarded as adjacent to the treatment group and therefore possibly impacted by spillover effects. The results for the 5–10 and 10–15 km rings using Method 1 are provided in Table 6. The results, similar to those obtained using the original control groups, confirm the negative effect of the announcement on more expensive neighbourhoods only (see Tables S23 and S24 for results from using Methods 2 and 3 and both rings). The results also show no evidence of spillover effects. This may be because the treatment suburbs are large enough to cover areas considered adjacent to the announced public housing locations and any sorting that has taken place is not systematically large enough to create a significant spillover effect within 1 year of the public housing announcement.

While our treatment groups in the DiD estimations are the host suburbs, we also estimated the same DiDs where our treatment groups are circles around the announced public housing with radii of 1, 1.5, 2 and 2.5 km. The control group is the 5 km-radius circle, excluding the smaller circle treatment groups. The results provided in Table 7 show that the average impact of the public housing announcement in the relatively more expensive host suburbs across these four treatment groups is a 5.2 per cent fall in property prices, and in the relatively less expensive suburbs there is no or negligible impact on property prices—for example, confirming the results shown in Table 5.

The results obtained using Methods 2 and 3 are similar to the above results obtained using Method 1—for each of the five control groups, for the more expensive suburbs, the impact of the announcement on nearby property prices is negative and for the less expensive suburbs the impact is none. Looking at the magnitude of the impact, for the property price drop near the more expensive suburbs, the magnitude obtained using Method 2 is slightly less than the Method 1 magnitude and, conversely, the magnitude obtained using Method 3 is slightly more than the Method 1 magnitude. The average drop across the five control groups is: 5.9 per cent using Method 1, 5.1 per cent using Method 2 and 6.6 per cent using Method 3. While the results obtained across the three methods are similar enough for our purpose, we note that the Method 1 results that we focus on lie between the results obtained from Methods 2 and 3.[18]

(iv) Robustness Checks

To further check the robustness of the results, we use different variations of our empirical models and different sample data. These results are provided in Tables S29–S40. We estimate different variations of hedonic models that include the physical and locational attributes and treat missing observations in different ways. For example, when we categorise properties by number of bedrooms and bathrooms, we mix the categories around in our various hedonic models. In the case of lot size, our base model includes lot size in five categories but we also estimate our models where log(lot size) is included as a continuous variable (Table S29). With regard to controlling for locational factors, while our base model controls for suburbs, we also estimate hedonic models where no suburb dummies are included (Tables S7–S11 and S18–S22).

With regard to addressing missingness, we use dummy variable techniques in our base models. We also check robustness by estimating the impact of the announcement on all treatment properties where we exclude properties with missing characteristics, and we find similar results (Table S30). With regard to the variations in control group

[18] Method 2 involves calculating residuals that are expected to be free of the implicit hedonic values of characteristics. However, the public housing announcement can be expected to impact these implicit values (e.g. lot size). This should constitute part of the announcement effect, but is taken out when calculating the estimated residuals, which results in the method understating the impact of the announcement on property prices. Method 3 allows the implicit hedonic values of characteristics to vary between treatment and control groups and pre- and post-announcement periods, whereas these are held fixed in Method 1. If, in relation to the control group, the post-announcement treatment properties are of lower quality (including lower hedonic valuations) than the pre-announcement treatment properties (making the treatment effect heterogeneous), then Method 1 may over-adjust the quality change for the post-announcement treatment properties which, in turn, would understate the public housing impact.

properties, we use properties in non-host suburbs that are in the same postcode as the host suburbs where the public housing will be located (Table S31). To check that our results are not impacted by the inclusion of the five ACT rural townships that are outside Canberra, we undertake further estimates using only suburbs within Canberra (Table S32).

While our analysis involves estimating DiDs for 1 year before and 1 year after the public housing announcement, to ascertain the impact for even shorter periods, we also conduct the analysis for 90, 180 and 270 days before and after the announcement. The estimated *Post* × *Treatment* coefficients for the relatively more expensive treatment group and for the 5 km-radius control group show evidence of panic sales immediately after the announcement leading to a 13.4 per cent drop in prices in the first 90 days post-announcement. The impact reduces over 180 days to a 10.7 per cent drop in prices, over 270 days to a 9.7 per cent drop and then over a year to a 5.4 per cent drop. The results for the less expensive treatment group show erratic results with estimated coefficients mostly economically and/or statistically insignificant. The results indicate that the negative impact for the more expensive group and no impact for the less expensive group remain for up to 1 year where our data end (see Tables S33–S35 for detailed results).

With regard to the variations in treatment group properties, we restrict the treatment group to properties sharing the same street where the public housing will be located. Because of the small sample size of treatment group properties, we were able to carry out cross-tabulation but could not use our econometric models for this exercise. Further variations in our selection of treatment group properties are based on the distance of the properties from the public housing locations and include distance dummies as the treatment variables in the DiD model. While we find that most of the estimated treatment coefficients are not significant, possibly due to fewer positive values, we generally find that the public housing impact on property prices reduces as distance increases (Table S36).

Additionally, we have estimated the treatment effects separately for the treatment suburbs. The results show that all the estimated DiD coefficients are negative for each of the three more expensive suburbs and each of the five control groups. While there are variations in the magnitude of the estimated DiD coefficients across the more expensive suburbs, the results show statistical significance at the 5 per cent level for all but one of the estimated coefficients. Among the less expensive suburbs, the signs of the estimated DiD coefficients are erratic for *Holder* and *Rivett*. All but two of these estimated coefficients have low magnitudes and are not significant at the 5 per cent level. We are unable to implement the DiD regressions for *Oxley* separately, given its very low sample size in relation to the large number of property characteristics, so we pool the *Oxley* and *Monash* transactions, the DiD results of which show positive but not significant estimated coefficients. Note that we have included *Oxley* (a non-host suburb) because the announced public housing in *Monash* is right beside the *Monash–Oxley* boundary (Table S37).

Since we have a small number of treatment group observations, especially when we conduct the analysis separately by more and less expensive suburbs, it might be the case that we do not have normally distributed errors. In this situation, randomisation inference (RI) can provide us with more accurate p-values as suggested in Canay et al. (2017) and Hagemann (2019). We conduct the RI test provided by Heß (2017), in which we randomly assign (false) treatment status and see the proportion (*p*-value) by which the estimated treatment effect is lower in absolute value than the false treatment coefficient (which should be zero). The *p*-values from the RI test confirm the conclusions that we reached from using Methods 1–3 (Table S38).

To conduct a more like with like comparison between the treatment and control groups, we identify the more expensive properties and less expensive properties within the original five control groups to form our new control groups. We then run DiD regressions where we compare the more expensive (original) treatment group properties with each of the newly identified more expensive control groups, and do likewise for the less expensive groups. The results show that for the more expensive groups, the estimated DiD coefficients are negative and significant at the 5 per cent level for 4–10, 15, 20 and 25 km—control groups, however, the magnitude of the estimated coefficients is smaller than the estimated coefficients of the original DiD regressions. As for the 5 km-radius control group, the results for the more expensive groups show no significance in the estimated coefficient, which could be due to small sample size. For the less expensive groups, the results show that the estimated coefficients are not significant at the 10 per cent level for all five control groups. The test indicates that our original results hold that the public housing announcement had a negative impact on nearby property prices in the more expensive suburbs and none to negligible impact in the less expensive suburbs (Table S39).

A question that may arise is whether the estimated DiD coefficients, instead of capturing the announcement effect, are capturing mean reversion in property prices. To examine this, we conduct a placebo test where we create artificial treatment and control groups within each of the previously defined five control groups of properties. We split each of the original control groups into three new groups based on property prices, with the top one-third prices in one group, the bottom one-third in another group and the middle (34 per cent exactly) in another. If the original results were tainted by mean reversion, the DiD regressions would show a negative impact when the top group is compared with the middle group and a positive impact when the bottom group is compared with the middle group. The results show statistical insignificance in all but two of the estimated coefficients, indicating mean reversion may not have played a role in the post-announcement price changes of the original groups (see Table S40).

(v) Validation of the Common Trend Assumption

To demonstrate the validity of the common trend assumption, we employed data with the same geographical coverage (the ACT) but going back for one more year, starting from January 2015. Using this data, we conducted placebo tests where we carried out the exercise we conducted in our base models but now with a false announcement date, 15 March 2016 (i.e. 1 year prior to the actual announcement). Specifically, we used properties sold between 15 March 2015 and 14 March 2016 as the pre-announcement sample and those sold between 15 March 2016 and 14 March 2017 as the post-announcement observations. Table 8 provides the results obtained for these placebo tests and shows that none of the estimated DiD coefficients are significant at the 5 per cent level, indicating the validity of the common trend assumption. We conclude similarly when we use the other two methods of identifying the treatment effect (Tables S41 and S42).

To further confirm that our estimated DiD coefficients are not affected by the failure of the parallel trend assumption, we adopted a simple hedonic imputation technique following Hill (2013), Hill and Syed (2016) and Prest et al. (2023). In this approach, we impute the prices of pre-announcement properties for the post-announcement period (for this, the pre-announcement property characteristics remain the same for pre- and post-announcement imputed prices) and similarly, we impute the prices of post-announcement properties for the pre-announcement period (for this, the post-announcement property characteristics remain the same for pre- and post-announcement imputed prices). To achieve this, for the control group, we run a hedonic regression using the pre-announcement period sample and use the estimated coefficients to predict the prices of the properties sold in the same period. Next, for the same control group, we run a hedonic regression using the post-announcement period data and use the estimated coefficients to predict the prices of the properties sold in the pre-announcement period. We repeat this process for the treatment group. Finally, using these four sets of predicted property prices, we use a canonical DiD model to estimate the impact of the announcement, given the pre-announcement period property characteristics. The same process is employed to estimate the impact of the announcement,

TABLE 8
Placebo (False Announcement Date) Test Results Using Method 1

	More expensive suburbs	Less expensive suburbs	All suburbs
(a) 5 km-radius control group			
Post (placebo)	0.050***	0.050***	0.050***
	(0.014)	(0.014)	(0.014)
Treatment × post (placebo)	0.008	−0.012	−0.001
	(0.024)	(0.025)	(0.020)
Adjusted R^2	0.48	0.48	0.48
N	1,780	1,734	1,945
(b) 10 km-radius control group			
Post (placebo)	0.048***	0.048***	0.048***
	(0.009)	(0.008)	(0.008)
Treatment × post (placebo)	0.004	−0.020	−0.011
	(0.021)	(0.028)	(0.021)
Adjusted R^2	0.55	0.55	0.55
N	3,072	3,200	3,411
(c) 15 km-radius control group			
Post (placebo)	0.052***	0.052***	0.052***
	(0.006)	(0.006)	(0.006)
Treatment × post (placebo)	0.000	−0.026	−0.016
	(0.018)	(0.028)	(0.021)
Adjusted R^2	0.63	0.62	0.62
N	5,341	5,469	5,680
(d) 20 km-radius control group			
Post (placebo)	0.051***	0.051***	0.051***
	(0.005)	(0.005)	(0.005)
Treatment × post (placebo)	0.001	−0.025	−0.015
	(0.015)	(0.027)	(0.020)
Adjusted R^2	0.64	0.63	0.63
N	7,790	7,918	8,129
(e) 25 km-radius control group			
Post (placebo)	0.053***	0.053***	0.053***
	(0.005)	(0.005)	(0.005)
Treatment × post (placebo)	−0.001	−0.027	−0.017
	(0.014)	(0.027)	(0.019)
Adjusted R^2	0.61	0.61	0.61
N	9,495	9,623	9,834

Note: (1) These placebo tests are based on a false public housing announcement in March 2016, that is 1 year before the actual announcement. (2) The regression models include a constant term, physical attributes of properties and suburb fixed effects. (3) The figures in parentheses refer to the standard errors of the estimates (clustered at the suburb level). (4) *, ** and *** refer to the significance levels 10 per cent, 5 per cent and 1 per cent, respectively.

given the post-announcement period property characteristics. Hence, the technique allows the pre- and post-announcement property characteristics to remain fixed while estimating the treatment effect, which is therefore free of any potential non-parallel trends arising from differences in property characteristics.

Results in Table 9 show that using pre-announcement property characteristics provides a slightly smaller (in absolute value) treatment effect (which is also less significant) for more expensive neighbourhood properties. For the same group, using post-announcement property characteristics provides us with a larger (and statistically significant) treatment effect. Finally, applying the method that pools both periods' property characteristics in the DiD regression (predictions are made for the transactions of both pre- and post-announcement periods) provides us with a statistically

TABLE 9
Difference-in-Differences (DiD) Regressions Results of Imputed Prices (Dependent Variable) Obtained From Stage 1 Hedonic Regressions

	More expensive suburbs	Less expensive suburbs	All suburbs
(a) Using pre-announcement property characteristics			
Post	0.056***	0.056***	0.056***
	(0.011)	(0.011)	(0.011)
Treatment × Post	−0.034*	0.010	−0.016
	(0.018)	(0.012)	(0.018)
Constant	13.369***	13.342***	13.360***
	(0.005)	(0.005)	(0.005)
Adjusted R^2	0.39	0.45	0.38
N	1,704	1,632	1,860
(b) Using post-announcement property characteristics			
Post	0.065***	0.065***	0.065***
	(0.012)	(0.012)	(0.012)
Treatment × Post	−0.071***	−0.011	−0.037*
	(0.014)	(0.012)	(0.018)
Constant	13.335***	13.314***	13.326***
	(0.005)	(0.005)	(0.005)
Adjusted R^2	0.42	0.44	0.40
N	1,838	1,890	2,066
(c) Using both pre- and post-announcement property characteristics			
Post	0.061***	0.061***	0.061***
	(0.011)	(0.011)	(0.011)
Treatment × Post	−0.039**	0.006	−0.021
	(0.018)	(0.011)	(0.017)
Constant	13.346***	13.332***	13.342***
	(0.005)	(0.005)	(0.005)
Adjusted R^2	0.42	0.45	0.42
N	3,366	3,294	3,522

Note: (1) Stage 1 of the method uses the estimated hedonic regressions in the previous table to impute a post-announcement price for each property sold in the pre-announcement period, a pre-announcement price for each property sold in the post-announcement period and a price for each property for the actual period (pre- or post-announcement) in which it was sold. (2) The figures in parentheses refer to the standard errors of the estimates. (3) The symbols *, ** and *** refer to the significance levels 10 per cent, 5 per cent and 1 per cent, respectively.

significant treatment effect of around 4 per cent, confirming an economically large negative effect of the announcement on more expensive neighbourhood properties. Following the same procedure, in no case do we see any impact on the less expensive neighbourhood properties. Hence, using a method that uses counterfactual property prices confirms our findings of the differential effect of the announcement, regardless of whether we use pre- or post-announcement property characteristics in the DiD regression.

VI Policy Implications and Conclusion

This paper investigates the differential effects of a public housing announcement on relatively more and less expensive host neighbourhoods. We took advantage of a single unanticipated government announcement of the construction of new similarly sized public housing in five locations in ACT, including both more and less expensive suburbs, and used a quasi-experimental framework with hedonic quality adjustment. We find no significant impact of the public housing announcement on relatively less expensive suburbs but a dampening effect of around 6 per cent in the first year after the announcement where the host suburb is relatively more expensive.

The observed impact on property prices in more expensive suburbs may have non-trivial economic implications. The 2016

Census data reveal the total stock of property in those suburbs was 9,116 (Australian Bureau of Statistics, 2016). Given the median price was AUD$600,000 in 2017 (see Table 1) and the 6 per cent relative fall in property prices remains constant at the margin, this implies a total loss in implicit property value of approximately AUD$330 million. Since property is a highly leveraged asset, the relative loss is substantially higher on the equity of the property owners. Using the average loan-to-value ratio of 50 per cent for Australia (Kohler & Hobday, 2019), this implies that the announcement resulted in these property owners missing out on a potential gain in equity of nearly 12 per cent within 1 year after the announcement.

The fall in equity will be even more substantial for new home buyers whose loan-to-value ratio is typically around 80 per cent, much higher than the national average. The large fall in property values will also result in a relative reduction in the amount of property tax collected by the ACT government. If, in contrast, all the public housing had been located in suburbs that are relatively less expensive than the remaining ACT suburbs, property owners in the host suburb would not experience any impact on their property prices; hence, the relative fall in property prices would not eventuate, and the amount of property tax collected by the ACT would be unaffected.

However, in addition to offering affordable shelter, public housing provides benefits to its residents that depend on the existing features of the host location; positive amenities, such as schools and playgrounds, will increase the benefit to new public housing residents (Gibbons et al., 2014; Breunig et al., 2019; Albouy et al., 2020; Kim, 2022) while negative features, such as crime and overcrowding, will have a detrimental effect on the residents (Gibbons, 2004; Sinai & Waldfogel, 2005; Eriksen & Rosenthal, 2010; Aliprantis & Hartley, 2015). These positive and negative location-based features also play an important role in determining the prices of properties across different suburbs, so the more expensive suburbs tend to have more amenities than the less expensive suburbs (Bayer et al., 2007; Ellen et al., 2007; Diamond, 2016). This means that if the public housing is located in the less expensive suburbs, the benefit to the future public housing residents will be lower than if it is located in the more expensive suburbs.

A question that plays an important role in policy discussions in welfare and regional economics is how to attain a balance between efficiency and equity (Alexiadis, 2018). Policy-makers may consider building public housing in the relatively less expensive suburbs because, ceteris paribus, this will maximise the combined market value of properties in the region. However, public housing in less expensive suburbs may result in economic and racial segregation (Verdugo & Toma, 2018; Harting & Radi, 2020; Hu & Liang, 2022). Hence, there is a trade-off between efficiency and equity —it may be more efficient for public housing to be located in less expensive suburbs but more equitable for public housing to be located in more expensive suburbs. Social planners who choose to maximise efficiency at a given level of equity will therefore choose the first option, and those who choose to maximise equity will select the other option (Kleinhans, 2004; Galster, 2007). Our analysis, looking at the differential impact of locating public housing in more and less expensive suburbs, will help policy-makers make informed decisions to achieve their desired balance between equity and efficiency.

In the case of the ACT, the socioeconomic advantage between the more and the less expensive suburbs is small (see Table S1). The host suburbs are located near each other in the southern part of the city, each roughly the same distance from the city centre; hence, the price difference due to any external amenities (i.e. amenities outside these treatment suburbs) would be negligible. Furthermore, Canberra is a relatively homogeneous city—the difference in the value of properties across suburbs may be partly due to the presence or absence of amenities. This means if social planners identify which particular amenities are responsible for the observed price differences (e.g. the presence or absence of good public transport or a proper playground), they could cover the cost of providing these amenities with the savings achieved by avoiding a relative fall in property prices through not locating public housing in the more expensive suburbs.

Informed social planners, desiring a balance between efficiency and equity, may locate public housing in relatively less expensive suburbs (on the grounds of efficiency) while also providing the amenities these suburbs lack (on the grounds of equity), resulting in an outward shift of the social welfare function. Given the resulting potential benefit of new amenities to host locations, which includes a potential rise in property prices (further efficiency gain), this kind of well-informed choice by policymakers may result in local residents seeing new public housing more favourably.

Conflict of Interest

None.

Supporting Information

Additional Supporting Information may be found in the online version of this article:

Appendix S1. Newspaper articles.
Appendix S2. Derivation of Method 3.
Appendix S3. Additional figures.
Appendix S4. Additional tables.

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